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THE CONCEPT OF THE REFLEX IN THE DESCRIPTION OF BEHAVIOR*

From the Psychological Laboratories of Harvard University

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INTRODUCTORY NOTE

The extension of the concept of the reflex to the description of the behavior of intact organisms is a common practice in modern theorizing. Nevertheless, we owe most of our knowledge of the reflex to investigators who have dealt only with "preparations," and who have never held themselves to be concerned with anything but a subsidiary function of the central nervous system. Doubtless, there is ample justification for the use of relatively simple systems in an early investigation. But it is true, nevertheless, that the concept of the reflex has not emerged unmarked by such a circumstance of its development. In its extension to the behavior of intact organisms, that is to say, the historical definition finds itself encumbered with what now appear to be superfluous interpretations.

The present paper examines the concept of the reflex and attempts to evaluate the historical definition. It undertakes eventually to frame an alternative definition, which is not wholly in despite of the historical usage. The reader will recognize a method of criticism first formulated with respect to scientific concepts by Ernst Mach (13, 14) and perhaps better stated by Henri Poincaré (18, 19). To the works of these men and to Bridgman's (1) excellent application of the method to more modern concepts the reader is referred for any discussion of the method *qua* method. Probably the chief advantage, first exploited in this respect by Mach, lies in the use of a historical approach. But the reader should understand that in the present case no attempt is made to give an exhaustive account of the history of the reflex. Certain historical facts are considered for two reasons: to discover the nature of the observations upon which the concept has been based, and to indicate the source of the incidental interpretations with which we are concerned.

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I

It was Descartes[1] who first proposed a mechanism by which the characteristics of the living organism could plausibly be produced. He came very near describing its action as the true mode of operation of the animal body, but the criticism of his contemporary, Nicolas Stensen, probably expressed his intention correctly. "Descartes," said Stensen, "was too clever in exposing the errors of current treatises on man to undertake the task of expounding the true structure of man. Therefore in his essay on Man he does not attempt such a delineation, but is content to describe a machine capable of performing all the functions of which man is capable." This interpretation is borne out by the text of the *Traité*, where, although parts of the anatomy are again and again pointed out as suitable for the functions of the mechanism, and the machine and body are, indeed, almost identified, the reader is, nevertheless, invited only to suppose the truth of the details. Descartes' interest lay primarily in furthering his philosophical notions, and the invention that is usually taken as the earliest expression of the reflex was little more than an instrument of persuasion.

In designing a convincing model of the living organism Descartes faced a peculiar difficulty. Movement in itself was easily enough obtained, for there were many mechanisms available as sources of energy. There was, for example, the current explanation of muscular contraction upon a hydraulic analogy, which, in fact, Descartes adopted. But if the energy itself was conveniently accounted for, the direction and order of its release were, on the contrary, critical. In meeting this difficulty Descartes introduced a novel device—the mechanism of the stimulus, by means of which external forces released the movements of the machine. The stimulus distinguished the model of *la bête machine* from that of a mere activated doll. It enabled the model to simulate the appropriateness and the apparent spontaneity of the movements of the living organism. So far as Descartes' purpose was concerned, it was successful in supplanting certain metaphysical concepts as causal agents leading to movement.

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[1]Descartes' account is to be found in the *Traité de l'homme* (4, pp. 130 ff.). The *Traité* differs in many respects from the earlier *Passions de l'âme* (3) in its representation of the action of the nervous system.

A convenient account of Descartes as physiologist is given by Sir Michael Foster (6). The quotation from Nicolas Stensen and the translations from Descartes are taken from this work.

Heretofore, the supposition had been that an animal moved because of the action of, let us say, a "soul." Descartes proposed that the body be regarded as a system of stored energy, and he pointed to minute, hitherto unobserved forces that acted upon the organism in such a way as to serve as releasing mechanisms.

The principle of the stimulus was, of course, little more than a guess. With an enthusiasm for the new physics, Descartes contended that the movements of an organism were functions of the forces acting upon it, but he could in practice point to only the roughest demonstration of this relationship. Subsequent investigation of the reflex has revealed the extraordinary difficulty of identifying the stimulating forces correlated with particular movements. The information available in Descartes' time was so scant, and the principle so far-reaching, that one is tempted to regard the discovery of the stimulus simply as another example of the insight with which Descartes anticipated later thought. But this would be to overlook the influence of an unusual analogy.

Descartes sought a mechanical model of the living organism for the support of an argument. For other reasons, namely, for the sake of the entertainment which they afforded, suitable models (utilizing the action of a stimulus as a source of spontaneity and appropriateness) had already been constructed by the engineers of the royal fountains in France. Descartes describes two of these fountain figures and the action of their releasing mechanisms, which are operated unwittingly by the observers.

> "For in entering they necessarily tread on certain tiles or plates, which are so disposed that if they approach a bathing Diana, they cause her to hide in the rose-bushes, and if they try to follow her, they cause a Neptune to come forward to meet them threatening them with his trident. Or if they pass in another direction they occasion the springing forward of a marine monster who spouts water into their faces or things of a like sort according to the caprice of the engineers who constructed them."

A contemporary engineer, Salomon de Caus,[2] published an account of the operation of similar figures, although he did not describe the two groups referred to by Descartes. The mechanical principles

[2]See (2). The date (1624) is considerably earlier than that of the *Traité*, and the examples treated by de Caus are, in general, simpler than those described by Descartes.

are few in number, but among them can be found all of those used by Descartes in his "reflex arc." In Descartes' proposed model the organ of sense is set in motion "even ever so little" by the external object and pulls upon a thread, which in turn, acting like a bell rope, opens a valve at a central reservoir, letting the contained fluid flow outward along a pipe-line into the muscles, which it activates. With a plate or lever substituted for the organ of sense and a water-wheel or similar device for the muscles, the description applies as well to the fountain figures. So slightly does Descartes depart from the details of the fountain mechanism that its position as the prototype of his model seems unquestionable.

It was the accident of a convenient analogy that led Descartes to the discovery of an important principle, and so great a mutation was it in the evolution of human thought that it proved lethal. In spite of frequent assertions to the contrary, Descartes seems to have exerted no influence upon the development of the reflex. Instead, the discovery of the stimulus was made again, with great difficulty, as the culmination of a century of experimentation, and another century and a half had elapsed before the principle had again been comparably extended to the behavior of the total organism. This lack of historical influence may be variously explained. Descartes was, as Foster has said, a "retrograde" physiologist, who accepted the more convenient theory, as against the more accurate, for the sake of a broader consistency. His interest was ultimately philosophical, even in his physiological explanations, and he did not attempt to discover the true action of the nervous system.

Descartes is important to an understanding of the reflex, not because of an organic connection with subsequent history, but as a symbol. The stimulus is an essential part of a mechanistic theory of behavior, whether the notion is arrived at through observation, as it was with Marshall Hall, for example, or argued from physical necessity or mechanical analogy, as it was with Descartes. Furthermore, the analysis of behavior that is accomplished in the mere descriptive phrase, "withdrawing the foot from fire," became a critical part of later method. But a further characteristic of Descartes' position must be noted: although he substituted the stimulus for a metaphysical concept in his description of the animal, Descartes could not eliminate metaphysical concepts from his description of man. Here he regarded the mechanical principles as at work, but under the control of the soul, which might suspend the physical necessities much as the engineer might modify the activity of the fountain figures.

Descartes reserved a field of action for the concept of soul, not because the physical facts were any more lacking in the case of man than elsewhere, but because of the pressure of certain metaphysical notions. Fragments of similar reservations still prevail. But the history of the reflex can almost be told by describing the progressive encroachment of the stimulus upon them. The line that Descartes drew between the fields of action of his physical and metaphysical concepts was a temporal one only. A movement might follow at one time the action of a stimulus and at another the action of soul. The later distinction that was first definitely established by Marshall Hall set in part an anatomical boundary. But both lines were drawn for the same purpose, namely, to resolve, by compromise, the conflict between an *observed necessity* and *preconceptions of freedom* in the behavior of organisms. In one form or another, this compromise accounts almost wholly for the aspects of the historical definition of the reflex that we are attempting to reconsider.

II

The concept of the reflex arose again from investigations which had already begun during Descartes' lifetime. They were concerned with animal movement and represented a sudden turn in the history of the concepts dealing with that phenomenon. It is a generalization sufficiently accurate for our purposes to say that the movement of an organism had generally been taken as coexistent with its life and as necessarily correlated with the action of some such entity as soul. The necessary relationship between the action of soul and the contraction of a muscle, for example, was explicit. As a consequence, it was disturbing to find, experimentally, that a muscle could be made to contract after it had been severed from a living organism or even after death. This, however, was the contention of the new physiology.

The demonstration that the volume of a muscle does not appreciably increase during contraction was made by Francis Glisson[3] in the middle of the seventeenth century. He was, as Fulton has shown, probably anticipated here by the Dutch naturalist, Swammerdam, whose influence was less immediately felt. Swammerdam's experi-

[3]For an account of the events antecedent to the explicit formulation of the concept of the reflex see: (*a*) Foster (6); (*b*) the historical introduction in Fulton (7); (*c*) Chap. 1 in Verworn (23); and, for many quotations from the sources, (*d*) Fearing (5).

ments on excised nerve and muscle were more clear-cut than those of Glisson on the intact limb, but either procedure was convincing and was something more than a mere disproof of the "animal spirits" hypothesis of muscular contraction. The experiment pointed to the existence of a "property of contractility" resident in the muscular tissue and independent of any remote source of energy. Contemporary and subsequent experimentation was of the same import. Glisson himself experimented upon intestinal and skeletal muscle after death, when metaphysical concepts had supposedly ceased to act. Swammerdam's experiments indicated a characteristic activity in excised nerve and muscle, and in 1700 Giorgio Baglivi, the Italian physician, reporting the contractions of isolated muscular tissue, emphasized that this was "without the soul's having any share in it, or even being sensible of it." The experiments published by von Haller in 1739 and 1742 permitted him to make the following claims:

> "By my experiments I separated this irritable nature on the one hand from a mere dead force, and on the other hand from the nervous force and from the power of the soul. I shewed that the movement of the heart and the irritable nature of the intestines depended on it alone. I confined it entirely to the muscular fibre. . . . I also shewed that that force was something perpetually living, and that it often broke out into movement though no external stimulus such as could be recognized by us was acting. By a stimulus, however, it could at any time be called back from rest into action. In a movement produced through it I distinguished between the stimulus which might be very slight, and the movement called forth by the stimulus which might be very powerful."

The doctrine of irritability was the theoretical accompaniment of this experimentation. As a property assigned to living tissue, irritability was from the first clearly defined in terms of the experimental operations which revealed it. In his fundamental experiment, Glisson noted that the gall bladder and the biliary duct bring about a greater secretion when they are irritated. "He argues," as Foster has noted, "that they cannot be irritated unless they possess the power of being irritated. This power of being irritated he proposes to denote by the term irritability." Although the concept was not immediately freed of non-physical counterparts, it was essentially a physical hypothesis, which ultimately led to the science of the general physiology of nerve and muscle. Movement, far from being the

objective manifestation of the activity of soul, had become an organic process subject to experimental investigation.

As Verworn has said, stimulation and irritability cannot be separated. Irritability, by its definition, implies the action of a stimulus. The doctrine of irritability, moreover, assigns an autonomy of function to the parts of an organism. These were the prerequisites for a formulation of the concept of the reflex. The first expression in harmony with the experimental material was made by Robert Whytt in a monograph first published in 1751. The genesis of the idea is apparent in a single sentence from that work. The observation is made that muscles will contract not only upon direct stimulation but *"whenever a stimulus is applied to the coats or membranes covering them, to the nerves which are sent to them, or to some neighboring or even distant part"* (24, p. 267).[4] Step by step, the point of stimulation recedes from the locus of the phenomenon with which it is identified. When the stimulus has been spatially distinguished from the response, the inference of a conducting medium is necessary, and a further experiment by Whytt, suggested to him by Stephen Hales, showed that the spinal marrow was a necessary part of this conducting path, which could not function if the marrow were destroyed. We shall need to return later to discuss the significance of these experiments and certain points of method which they exemplify.

Whytt, it is true, regarded the conducting force as a "sentient principle," which, if it was non-rational, was also non-physical. He thus followed the example of Glisson in his compromise with older concepts. Nevertheless, the observations themselves were independent of Whytt's interpretation. His psychical qualifications, moreover, were in this sense useful: that they permitted him to generalize his principle to various functions of the intact organism, free of the resistance that would have been encountered if the principle had been wholly physical. The application to the vegetative functions was easily made, for it had already been strongly foreshadowed. He extended his doctrine also to the field of action of some of the departments of the soul.

III

Whytt left the concept of reflex action very much as Glisson had

[4]The quotation is from the second edition of this, published in 1763, page 267. The first edition omits "to the nerves which are sent to them."

left irritability: in the position partly of a description of observed fact and partly of a superfluous interpretation. What von Haller had done in establishing irritability as a physiological datum independent of any aspect of soul, Marshall Hall[5] now repeated on behalf of the reflex. Not only, he suggested, could muscular tissue contract solely by virtue of its property of contractility, but a given muscle *in situ* could be brought into action by a train of nervous events which were in themselves acting only by virtue of an intrinsic property. "This principle," he said, "is that termed *vis nervosa* by Haller, *motorische Kraft* or *vis motoria* by Professor Müller, and *excitabilité* by M. Flourens."[6] To describe the series of events that a single instance of this activity comprised Hall accepted the word reflex, and he spoke of the principle in general as "the reflex function."

The hypothesis that the phenomena of the reflex arc are only aspects of irritability seems to have been original with Marshall Hall. Of the contributions that he claimed to have made it resisted most successfully the charge of plagiarism leveled against him by his contemporaries and was most often cited in his defense. The hypothesis involved, not a denial of the operation of non-physical concepts, but an exclusion of their operation from the reflex field. In Hall's own estimation, as Sherrington[7] has pointed out, "his chief advance lay in the doctrine of separateness in the central nervous system of the great sub-system for unconscious reflex action, and another great sub-system for sensation and volition." This estimate of himself has been more or less confirmed historically. He is, it seems fair to say, the acknowledged author of the almost immutable distinction between voluntary and reflex action and of the resulting negative definition of the reflex as a form of movement *un*conscious, *in*voluntary, and *un*learned. The emphasis that this brief account has thus far placed upon the relation of the reflex to non-physical concepts may now appear more reasonable. Hall's basic hypothesis was simply a restatement of the relationship and must be understood accordingly.

Hall distinguished between four modes of muscular action. *Voli-*

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[5]See (8).

[6]From the *Memoir* of 1837 (9), page 44. Whytt had distinguished between the action of the will and the action of a stimulus, but by the latter he probably intended only "mechanical action" upon a muscle. Whytt also used the word "spontaneous" to aid in the distinction, but in the reverse sense! *Reflex* movements were for him spontaneous, since they are performed by the several organs "as it were of their own accord."

[7]From Stirling (22, p. 86).

tion, he said, acts through the cerebrum, is spontaneous, and affects the muscles in a direct line through the spinal marrow and motor nerves. *Respiration* acts in the same way, is also spontaneous (sic), but its seat is in the medulla. *Involuntary* action is the response of a muscle to direct stimulation (the phenomenon of irritability). The fourth mode (the *reflex function*) involves the spinal marrow and differs from volition and respiration in that it is neither spontaneous nor direct in its course.

> "It is, on the contrary, *excited* by the application of appropriate stimuli, which are not, however, applied immediately to the muscular or nervo-muscular fibre, but to certain membranous parts, whence the impression is carried to the medulla (spinalis), *reflected,* and reconducted to the part impressed, or conducted to a part remote from it, in which muscular contraction is effected."

We may neglect the second class, which seems to have been included because of the current knowledge of the respiratory center but was exclusive of neither voluntary nor reflex action, and the third (unfortunately called involuntary), which was only a statement of irritability. For the distinction between the two remaining classes (volition and reflex action), we may consider Hall's available evidence.

Hall defined volition as a form of movement which was (*a*) spontaneous and (*b*) dependent upon the integrity of the cerebrum. The second characteristic was not in itself sufficiently distinguishing. Hall was familiar with the segmental nature of the spinal functions, and it was a fair supposition that the differential activity found in so uniform a structure might be accentuated in the higher segments. If it was a matter of observation that characteristic movements of the animal disappeared upon ablation of the cerebrum, this was also true for any given part of the cord. Spontaneity, on the other hand, was the more critical factor in the delimitation of volition. But spontaneity, as Hall used it, described in effect only those movements for which no appropriate stimuli could be observed, and the word seems to have had no other meaning. The distinction between reflex and voluntary action rested, then, upon the possibility or the impossibility of the experimental demonstration of stimulating forces.

To the support of his distinction Hall mustered a variety of other facts. He noted that drugs (such as opium or strychnine) discrimi-

nate in their action between voluntary and reflex activity, that the brain sleeps, while the spinal marrow never sleeps, and so on. He was, of course, describing an observable phenomenon, which we recognize as the differential functioning of separate reflex systems.[8] The observations do not, however, indicate an essential difference between brain and cord, especially since the brain-cord distinction is seldom strictly respected physiologically. Hall's appeal to physiology, like that to anatomy, lacked cogency. His distinction rested primarily upon the single item of spontaneity.

In defining volition as the hypothetical antecedent of movement for which no corresponding stimulus could be observed, Hall left the concept open to extensive modification, for it was implicit in the nature of the reflex that it should, in the course of its growth, disfranchise volition. So far as it concerns behavior, the history of the reflex has been, in fact, essentially the account of the discovery of stimuli and of the concurrent passage of the corresponding behavior from the field of volition into the field of reflex action. Furthermore, in opposing volition and reflex action as mutually exclusive terms, Hall identified the reflex with scientific necessity, and volition with unpredictability. This was the pattern for future controversy, of which we may note two instances.

In 1853 Pflüger,[9] as is well known, questioned the reflex nature of the movements of the spinal frog on the basis of unpredictability. In separate instances of the flexion reflex, he pointed out, the movement of the leg varies widely, although the stimulus is held constant. On the basis of the observed variability, Pflüger postulated a spinal mind, his famous *Rückenmarkseele*. Note that the experimental justification for mind (as for Hall's volition) was the absence of demonstrable necessity, that the function of the non-physical concept was, as heretofore, to account for variability. Refutation of Pflüger's criticism needed only a demonstration that the observed variability was itself a function of collateral stimulation. The necessary observations were first supplied by von Uexküll, and the principle (*Shaltung*) has been elaborated by Magnus (15). Briefly, they have shown that a given response may be modified through

[8] An exhaustive account of the discriminating action of drugs upon separate reflex systems is available in Magnus (15, pp. 643 ff.).

[9] See (17). This controversy, of course, extended well beyond Pflüger's personal participation. It was, in fact, the continuation of a philosophical reaction against Hall's concept and was only accentuated by Pflüger's interpretation of his experimental findings.

proprioceptive stimulation arising from the posture of the animal. The effects that Pflüger observed in the spinal frog are consequently subject to adequate prediction, and, in this particular instance at least, the variability has disappeared. With it disappeared also its corresponding *Seele.*

Shortly before the beginning of the century Pavlov (16) was engaged in the investigation of the activity of the digestive glands. For much of this activity it was possible to identify the necessary antecedent events (the mechanical or chemical changes acting directly or reflexly upon the glands). The greater part of the normal secretion, however, was unfortunately not under the control of the experimenter. The reader should not be surprised that this was called "psychic" secretion. Pavlov undertook the investigation of this activity. His findings are too well known to call for more than the briefest comment here. Essentially, it was the discovery of the operation of "substitute" stimuli. The nature of the process of substitution (conditioning) and the use of the principle as a method are not important at this point. We may emphasize, however, the aspects of the discovery which exemplify the usual course of reflex investigation. *Given a particular part of the behavior of an organism hitherto regarded as unpredictable* (and probably, as a consequence, assigned to non-physical factors), *the investigator seeks out the antecedent changes with which the activity is correlated and establishes the conditions of the correlation.* He thus establishes, as we say, the reflex nature of the behavior. In traditional practice, upon the demonstration of such a correlation, non-physical concepts dealing with the same subject-matter are discarded.

IV

The subsequent development of Hall's formulation could by its very nature approach only one end, namely, the hypothesis that the total behavior of the intact organism might be described in terms of the reflex. This extension was possible only upon the demonstration of necessary correlations in a large body of residual behavior, most of which was mediated by the distance receptors of the head segments. The work of Pavlov (16) may therefore be taken as historically fundamental. His evidence was decisive, if necessarily incomplete. It led to two achievements. The principle of conditioning supplied the extended range of stimulation needed to account for the complex behavior of the total organism, and the demonstration

of the reflex activity of the cortex laid siege to the last stronghold of the old anatomical distinctions. The extension of the concept has been further facilitated by the work of Magnus upon reflexes concerned with the maintenance of posture. Magnus (15), like Pavlov, broadened the field of operation of the stimulus by discovering a large number of specific stimulus-response correlations, and, again like Pavlov, he attacked the anatomical distinctions by demonstrating the reflex nature of the activity of the higher, although chiefly subcortical, centers. The reflex as a concept in the description of behavior has received its most extended systematic support from behaviorism.

The adequacy or inadequacy of the reflex in the description of total behavior seems to be beyond immediate experimental demonstration and, in any event, is beyond the scope of this paper. Some of its implications also need not greatly concern us, as, for example, the fate of the non-physical concepts that are deprived of their field of operation. We may regard the ultimate validity of the concept of volition (as, indeed, of that of the reflex) as beyond any immediate estimation. We are concerned with the reflex as a working concept. What is its nature and how shall it be defined? In particular, we have set ourselves to resolve certain difficulties of definition imposed by the extension to total behavior, where volition (or the practice that it represents) is important for its effects. But perhaps we have reviewed enough of its history and may turn directly to a statement of the argument.

In the history of the reflex one positive characteristic has always been given by the facts—the observed correlation of the activity of an effector (i.e., a response) with the observed forces affecting a receptor (i.e., a stimulus). The negative characteristics, on the other hand, which describe the reflex as involuntary, unlearned, unconscious, or restricted to special neural paths, have proceeded from unscientific presuppositions concerning the behavior of organisms. When Marshall Hall decapitated his famous newt,[10] he pointed quite correctly to the reflex activity of the parts of the headless body, to the observed fact that movement followed, inevitably, the administration of specific stimuli. But his assumption that he had imprisoned in the head of the newt the source of another kind of movement was irrelevant and unsupported. The fact before him was a

[10]Hall first described the behavior of the decapitated newt before the Zoölogical Society on November 27, 1832. See (9, p. v).

demonstrable necessity in the movement of the headless body; his failure to observe similar necessities in the movement of the intact organism was the accident of his times and of his capabilities.

Tentatively, then, we may define a reflex as an observed correlation of stimulus and response. When we say, for example, that Robert Whytt discovered the pupillary reflex,[11] we do not mean that he discovered either the contraction of the iris or the impingement of light upon the retina, but rather that he first stated the necessary relationship between these two events. So far as behavior is concerned, the pupillary reflex is nothing more than this relationship. Once given a specific stimulus-response correlation, we may, of course, investigate the physiological facts of its mediation. The information there revealed will supplement our definition, but it will not affect the status of the reflex as a correlation. These are matters, however, that will bear a more detailed treatment, for they present many problems.

V

The notions of both stimulus and response were, as we have seen, essential to the principle of irritability, so that the correlation that we are emphasizing was already present (in its most easily observed form) in the older concept. The reflex emerged as a separate principle when a correlated stimulus and response could be spatially distinguished, and we have already commented upon Whytt's insistence upon the possibility of a spatial differentiation. The observation of a correlation between two spatially discrete activities led at once to the inference of a series of intervening events, to the inference, that is, of conduction. Subsequently, the investigation of the events intervening between a stimulus and its correlated response became the particular field of reflex physiology. We shall need to review certain characteristics of its method, and we may turn first to the procedure by which the anatomical structures underlying the mediation of a reflex are identified.

Even for so early an investigator as Robert Whytt convenient material from the physiology of the nervous system was available in interpreting reflex phenomena. Rough descriptions of the activity of end-organ and effector and of the conducting action of nerve were at hand. The investigators of the reflex appropriated this current

[11]The text disregards the supposed discovery by Galen and Descartes.

knowledge but began immediately to refine the references to anatomy. Whytt (24), as we have already seen, first demonstrated that the necessary relationship between stimulus and response (or, as he expressed it, the "sympathy between different muscles or other parts of the body") was lacking after destruction of the cord; "from whence it seems to follow," he added, "that the nerves . . . . have no communication but at their termination in the brain or spinal marrow." His conclusion, if it was not strictly logical, was made extremely probable by Bell's subsequent differentiation between the functions of the anterior and posterior spinal roots. Although Bell did not expressly subscribe to the reflex doctrine, his experiments are more to the point in its support than in his generalization to "sensation and the power of motion." The "diastaltic arc" of Marshall Hall (10, pp. 6 ff.) applied Bell's discovery more explicitly to the principle of the reflex.

The diastaltic arc (eventually spoken of as the reflex arc) embraced an "esodic" (afferent) nerve, a spinal center, and an "exodic" (efferent) nerve. It was an *anatomical* term. The experimental evidence for its close correspondence to the reflex was of the sort we have noted: the impairment of reflex function after anatomical injury (Whytt), the fractional functioning of surgically isolated parts of the arc (Bell), and so on. Subsequently (especially in the neuron theory), the argument was extended to miscroscopic levels. By similar procedures the gross location of the central part of a given arc is determined. The practice is essential to the doctrine of the segmental action of the cord and was utilized early in the history of nerve physiology (independently of reflex theory) in the localization of higher centers, e.g., the respiratory center. Typical examples in reflex investigation may be found in the work of Magnus on various mid-brain preparations. The basic assumption is that, when the ablation of a particular part of the nervous system impairs or abolishes a reflex function, the ablated structure is essential to the reflex and includes part of the arc. That inference, of course, is not unavoidable. Moreover, in the interpretation of such experiments the probability of operative artifact must unfortunately be regarded as proportional to the degree of specificity desired. For the broader inferences, however, such as that of the participation of the spinal cord, the probability of serious artifact is negligible.

The notion of the reflex arc as the anatomical counterpart of a reflex has been generally accepted. An end-organ (or a nerve trunk

acting in that capacity), an afferent nerve, an interconnection between nerves in the cord or brain, an efferent nerve, and an effector are usually regarded as essential to the mediation of a reflex. We shall have no occasion to go beyond these rather general assumptions. The method that we have referred to is not peculiar to the reflex, and we shall not need to estimate it more closely. We may note, however, that the description of a reflex in functional terms (as a correlation of stimulus and response) is always prior to the description of its arc. In any available procedure the anatomical inference must always be drawn from an experiment in which the integrity of a *function* is critical.

In its simplest form, the concept of the reflex arc satisfies the need for continuity between stimulus and response, but the arc must serve as a locus not only for a communication between end-organ and effector but for modifications in the form of the communication. The statement of any reflex (for example: "the flexion of a limb following electrical stimulation of the skin of the foot") implies the possibility of a quantitative description of both stimulus and response. The statement thus expresses the observed correlation of two events, but by describing these events it describes also the special conditions of the correlation. A given stimulus and its response differ, for example, in time of inception, in duration, and in the form and amount of energy; and these modifications and conversions must be accounted for by the intervening events.

By procedures of the sort we have already described, the characteristics of a reflex have been assigned to particular parts of the arc. The gross conversions of energy have, of course, been referred to end-organ and effector, part of the elapsed time to afferent and efferent nerve, and so on. By a process of logical and surgical isolation, however, a certain group of the conditions of a reflex correlation have been shown to be independent of the activity of end-organ, effector, and nerve-trunk. These are the special characteristics of reflex conduction. They have been classically described by Sherrington (21). In Sherrington's list each characteristic is expressed as a difference between nerve-trunk and reflex-arc conduction, which means simply that certain of the inferences noted above have already been made. In reproducing the list here, we shall reword it in order to emphasize the nature of each item as an observed condition of a correlation of stimulus and response.

(1) There is a latent period between application of the stimulus

and appearance of the end effect; (2) the duration of the response is greater than the duration of the stimulus; (3) if stimuli are applied rhythmically (between certain limiting rates), the rhythm of the response does not closely correspond with the rhythm of the stimulus; (4) the intensity of the response does not vary rectilinearly with the intensity of the stimulus; (5) a single brief stimulus is often not effective, but succeeding stimuli following closely upon it are so; (6) afferent and efferent paths cannot function interchangeably; (7) repetition of a stimulus (with certain time specifications) evokes progressively weaker responses; (8) the strength of stimulus just sufficing to elicit a response is variable; (9) (*a*) a second stimulus is ineffective for a short interval after a first and subnormally effective for a succeeding short interval, (*b*) two stimuli at separate points of stimulation may facilitate each other, (*c*) a stimulus may act to produce the absence of a response, (*d*) injury to the nervous system may temporarily destroy or weaken the effectiveness of a stimulus which is subsequently found to be normally effective; (10) the effectiveness of a stimulus depends upon the integrity of the blood supply; (11) the effectiveness of a stimulus is partially or wholly abolished by anaesthetics.

Although all the characteristics except (6) and parts of (9) are represented to some extent in nerve conduction, the degree to which they are present in reflex conduction cannot be explained by reference to nerve-trunk alone. It has been assumed, therefore, that the characteristics represent the functioning of a special structure, which has been called the *synapse,* and which has been hypothetically located at the interconnection between neurons. Since these interconnections are grouped together in the gray matter of the cord and brain, the experimental practice associated with the other parts of the arc may be used in testing the hypothesis. It is possible, for example, to show that the synaptic characteristics are present in conduction through the cord when an afferent nerve is stimulated close to the cord and the response taken electrically from the efferent root. Again, the characteristics can be shown to vary with the temperature of the cord, but they are almost wholly independent of the temperature of either afferent or efferent nerve. Examples could be multiplied indefinitely. Moreover, as Sherrington has shown, the location of the synapse at the interconnection between neurons receives considerable support from the histology of the nervous system, from studies in nerve degeneration, from the physi-

cal chemistry of surfaces of separation, and from various other sources.

Reflex physiology seeks a physico-chemical description of the events peculiar to the mediation of a stimulus-response correlation. It regards the synapse, therefore, as a physico-chemical system. Theories of the details of that system have been of various sorts. Keith Lucas (12) sought an explanation of synaptic phenomena in terms common to nerve-trunk conduction. In his theory the synapse was regarded as a region of impaired conduction, and its description in physico-chemical terms waited only upon the description of the conduction of the nervous impulse. Sherrington (20), as is well known, has recently regarded the synapse as the locus for the depositing of excitatory and inhibitory substances or states. For Lapicque (11) the synapse is the boundary between neural structures of independent chronaxies. We shall have no need of evaluating theories of this sort, nor shall we find it necessary, in the light of our brief examination of method, to justify or discredit the hypothesis of the synapse itself. We are interested, not in the validity of that concept, but in its nature. Here we are led to one conclusion.

Our present information concerning the synapse is derived wholly from observed instances of reflex conduction. There is nothing in our description of the synapse which has not already served to describe experimental data, but we translate our descriptions of data into the laws of the synapse for convenience of expression. The synapse, that is to say, described in terms of its characteristics, is a construct. *It is the conceptual expression for the conditions of correlation of a stimulus and response, where the incidental conditions imposed by a particular stimulus and a particular response have been eliminated.*

There is nothing in the physiology of the reflex which calls in question the nature of the reflex as a correlation, because there is nothing to be found there that has any significance beyond a description of the conditions of a correlation. It may be objected that, should reflex physiology succeed in describing the synapse as a physico-chemical system, the synapse would be no longer conceptual. Actually, the description would be translated into concepts of another order, which would possess the tremendous advantage of being common to all the physical sciences. But we are here very close to certain fundamental questions of scientific method, which we shall not attempt to answer.

The physiological study of the reflex supplements and restricts our definition. It begins by identifying and describing certain of the

events which intervene typically between stimulus and response, and it then arbitrarily restricts the use of the word reflex to correlations which employ that kind of event. Physiologically, the word implies the participation of at least two neurons with a synaptic junction. The best practical criterion is irreversibility of conduction, which is not in any degree a characteristic of nerve-trunk conduction and is therefore the clearest evidence of the operation of a synapse. This restriction in the use of the word reflex excludes (1) the movement of the organism solely under the influence of mechanical forces (for example, the movement of the paw of a dog when it is "shaken"); (2) the activity of an effector in response to direct stimulation, a distinction that is more often of importance in dealing with the internal economy of the organism; (3) those responses mediated by other types of nervous system than the synaptic; and (4) those correlations between discrete activities which are mediated by non-nervous mechanisms, as, for example, by autacoids.

The advantage of these qualifications (even of the third, which is sometimes felt to be a notable difficulty) is that they insure a uniformity of material in the investigation of the reflex. It would be impossible to state with any degree of specificity a law describing the course of reflex fatigue if it were to apply to the exhaustion of such diverse mechanisms as a neural structure and a concentration of hormonic substance. The restricted definition limits the application of the principle of the reflex, but within boundaries which may be justified upon independent grounds.

We have tried to emphasize an essential continuity between reflex physiology and the special science of the description of behavior. We must not, however, fail to recognize a well-grounded distinction between the two fields, which is based primarily upon a difference in immediate purpose. The one seeks a description of the reflex in terms of physico-chemical events, the other a description of behavior in terms of the reflex. It is assumed that the word reflex refers to the same thing in both instances. Historically, however, the investigation of the physiology of the reflex proceeded, as we have seen, almost independently of the description of behavior and was advanced almost to its present status at a time when the possibility of a quantitative description of behavior was considered too remote for scientific consideration. The concepts of reflex physiology, consequently, cannot be transposed to the description of behavior without modification, which will in most cases be found, however, to pro-

ceed naturally from an analysis of the physiological method, as, for example, in the discussion of the synapse given above. Moreover, many assumptions which are unimportant and therefore tacit in reflex physiology are critical in the description of behavior and must be given an explicit and independent statement. This is true of the procedure by which a single reflex is isolated, as we shall see later. Again, a number of problems which arise in the description of behavior have no parallel at all in reflex physiology. Some of these matters we have already touched upon; others will enter at one time or another into the following discussion.

VI

We may summarize this much of the argument in the following way. A reflex is defined as an observed correlation of two events, a stimulus and a response. A survey of the history discloses no other characteristic upon which a definition can legitimately be based. The physiological investigation does not question the correlative nature of the reflex, for its data and its concepts deal essentially with the conditions of a correlation; but heterogeneous instances of correlations which would be embraced by the definition, read literally, are excluded by the physiological refinements of usage. It now remains for us to deal more specifically with the reflex in the description of behavior. What is the description of behavior, and how does the reflex, as a correlation, enter into it? Here (the reader may again be warned) we shall be concerned not so much with the validity or the adequacy of the concept as with its nature and the method peculiar to it.

Lacking some arbitrary distinction, the term behavior must include the total activity of the organism—the functioning of all its parts. Obviously, its proper application is much less general, but it is difficult to reach any clear distinction. The definition of the subject-matter of any science, however, is determined largely by the interest of the scientist, and this will be our safest rule here. We are interested primarily in the movement of an organism in some frame of reference. We are interested in any internal change which has an observable and significant effect upon this movement. In special cases we are directly interested in glandular activity, but this will usually concern us only secondarily in its effect upon movement. The unity and internal consistency of this subject-matter is historical: we are interested, that is to say, in what the organism *does*.

But the description of behavior, if it is to be either scientific or satisfying, must go further. As a scientific discipline, it must describe the event not only for itself but in its relation to other events; and, in point of satisfaction, it must *explain*. These are essentially identical activities. In the brief survey at the beginning of this paper it was occasionally necessary to regard the stimulus as a newly discovered cause of movement for which various conceptual causes had previously been designed. In this way we represented a real aspect of the history of the reflex. But we may now take that more humble view of explanation and causation which seems to have been first suggested by Mach and is now a common characteristic of scientific thought, wherein, in a word, explanation is reduced to description and the notion of the function substituted for that of causation. The full description of an event is taken to include a description of its functional relationship with antecedent events. In the description of behavior we are interested in the relationships within a regressive series of events extending from the behavior itself to those energy changes at the periphery which we designate as stimuli. We stop here in the regression only because further steps are beyond the field of behavior. The two end events, the behavior and the stimulus, have, moreover, a particular importance, because they alone are directly observable in an intact organism, and because they limit the series. With the relationship of these two end terms the description of behavior is chiefly concerned.

The reflex is important in the description of behavior because it is by definition a statement of the *necessity* of this relationship. The demonstration of the necessity is ultimately a matter of observation: a given response is observed invariably to follow a given stimulus, or exceptions to this rule may be independently described. In its extension to total behavior the principle generalizes the statement of the necessity observed in a particular reflex, the form of the expression remaining essentially the same. That is to say, the hypothesis that "the behavior of an organism is an exact, if involved, function of the forces acting upon the organism" states the correlation of a stimulus and a response, both of which remain wholly undifferentiated. It is, in this sense, the broadest possible statement of a reflex, but it is not an observed correlation and is therefore a hypothesis only.

It is, nevertheless, solely the fault of our method that we cannot deal directly with this single correlation between behavior as a whole

and all the forces acting upon the organism stated in the hypothesis. Quantitative statements of both stimulus and response and a statistical demonstration of the correlation are theoretically possible but would be wholly unmanageable. We are led, for lack of a better approach, to investigate the correlation of parts of the stimulus with parts of the response. For the sake of a greater facility (and in this case the very possibility) of description, we turn to analysis.

Originally, the use of analysis was quite accidental and unrecognized, but it has, nevertheless, always been necessary. The early observations were possible only after it had been achieved in some form or other. This is not difficult to understand if we remember that the correlation that we call a reflex rests ultimately upon observation. In an intact newt, to return to Hall's experiment, it would have been very nearly impossible to observe a correlation between the movement of the tail and the application of a probing needle, because the movement of the tail was also correlated with other stimuli and the action of the probing needle with other movements. *In the isolated tail,* however, one kind of movement followed a given stimulus and was absent in the absence of the stimulus. The correlation was obvious and therefore observed.

Marshall Hall and his few predecessors divided the behavior of an organism into parts by the expedient method of dividing the organism. This became, in general, the method of reflex physiology, although, for obvious reasons, the division of the nervous system supplanted the division of the whole organism. The best-known group of reflexes to be studied in surgical isolation are those surviving in the body of the organism after section of the cord just below the bulb. This is the "spinal" preparation, which has been the basis for the greater part of the physiological investigation, notably that of Sherrington. Other common reflex systems are the decerebrate, in which the medulla and the cerebellum remain intact, and the various mid-brain and thalamic preparations, as, for example, those of Magnus. A further extension of the method involves the surgical or physiological exclusion of end-organs, as by extirpation or anaesthetization (for example, of the labyrinth), or by section of afferent nerves. The common object of these procedures is to permit the investigation of a particular response in relation to a controlled variable, independent of other variables also related to that response.

But the same result may be obtained in another way. The ex-

periment may be so designed that the undesired variables do not vary. The distinction between the two methods will appear in the following example from the work of Magnus (15). Certain postural effects in a mid-brain animal are correlated partly with the position of the labyrinths relative to the earth and partly with the condition of flexion or extension of the muscles of the neck. The correlation between the posture and the state of the neck muscles can be studied alone if the labyrinths are cocainized or extirpated. But Magnus was also able to obtain the isolation by designing his experiments in such a way that the position of the labyrinths relative to the earth did not change. Perhaps the best examples of this method, however, are to be found in the work of Pavlov (16). Here the organism is intact and the very active receptors of the head segments fully functional. By controlling light, sound, odor, and other conditions in the experimental chamber, it is possible to observe in isolation the correlation between a given response and a selected stimulus. Placing an animal in a dark room, that is to say, is equivalent for purposes of isolation to blinding it, to sectioning the optic tracts, or to destroying the visual projection areas in the cortex, and has the great advantage over these surgical methods of being relatively free from unknown artifacts.

The practical merits of both these methods are obvious; but we are concerned with a broader aspect of analysis. For the physiologist, the isolation of a reflex is a preliminary matter of method and is relatively insignificant. In the description of behavior it is of first importance. How legitimate, then, is the process of analysis, and what is the nature of its product?

Let us deal entirely with the flexion reflex in the spinal dog, as a familiar and convenient example. We have already analyzed, of course, when we have once named, so that we must go back for a moment to the behavior as a whole. Without regard to its correlation with stimulating forces, behavior, as we have seen, is simply part of the total functioning of the organism. The problem of analysis at this level is common to physiology and anatomy. We shall not need to solve it, but shall assume that for purposes of description the body of an organism may be divided into parts (that we may speak, for example, of a leg), and that the functioning of a particular part may be described in isolation (that we may speak, for example, of the flexion of a leg). Moreover, we shall assume that the forces acting upon the organism may be analyzed and described in

the manner common to the physical sciences. Our own problem lies beyond these assumptions.

In the flexion reflex our first experimental datum is the nearly simultaneous occurrence of the flexion of a leg and, let us say, the electrical stimulation of the skin of the foot. If we measure both events very carefully and repeat the stimulation, we obtain a second flexion which closely resembles the first, and we find that we may corroborate the observation, within limits, as often as we like. We call the observed correlation a reflex and, for convenience of reference, give it a special name, the flexion reflex.

The question then arises: *what is the flexion reflex?* If we try to answer by describing in detail a stimulus and a response, we meet embarrassing difficulties. We find that the exact degree and direction of flexion may vary with many factors. We find, for example, that it was very important for our original measurements that the torso of the animal had a particular position, that the contralateral leg was, say, unsupported, and so on. But we cannot specify these incidental conditions in our description without destroying its generality. Thereupon we shall probably resort to surgical methods. Theoretically, at least, we may pare down the structures underlying the flexion reflex until the collateral variables are no longer effective. But we can never be sure that the reflex that we have thus carved out of the behavior of the organism would not have been grossly otherwise if our operative procedure had been different. We are not sure, that is to say, that what turns up at the end of our process of isolation is the flexion reflex. There is another method open to us. In the flexion reflex we are dealing essentially with a group of correlations showing many characteristics in common. They involve the same effectors acting roughly in the same way and stimuli which resemble each other at least in their gross anatomical reference. We may, therefore, if we wish, *construct* a flexion reflex by a statistical treatment of many of these separate correlations. We may, in other words, determine and state a correlation between the characteristics common to all our observed responses and the characteristics common to all our observed stimuli, and we may name this construct the flexion reflex. But the resulting description of this statistical entity will likewise depend upon our choice of observations and upon our method of analysis.

We have been proceeding, of course, upon an unnecessary assumption, namely, that there *is* a flexion reflex, which exists independently

of our observations, and which our observations approximate. Such an assumption is wholly gratuitous, but it is remarkably insistent. It arises in part from the nature of the reflex. If we remain at the level of our observations, we must recognize a reflex as a correlation. But the immediate uncritical reaction to a definition on that basis is that a correlation, in point of satisfaction, is not enough. There is an urge toward solidification, clearly evident throughout the history. We turn instantly to the reflex arc for material support. Although our knowledge of the critical part of the arc is, as we have seen, derived wholly from the observation of a correlation, we much prefer to regard the characteristics of the correlation as properties of the synapse than to retain them simply as characteristics of a correlation. Under the same pressure, then, but with less justification, we are led to assume that there are isolated reflexes concealed in the behavior of an organism, which by proper investigatory methods we may discover, and in the description of behavior to state the corollary of this proposition, namely, that behavior is the sum or the integration of these units.

Here we are touching upon the subject of a widespread current controversy, but we may, by virtue of what we have already said, dispose of the matter briefly. Let us phrase two typical questions. Is a reflex a unitary mechanism? Is behavior a sum of such mechanisms? Then, if by reflex we mean a hypothetical entity which exists apart from our observations but which our observations are assumed to approach, the questions are academic and need not detain us; if, on the other hand, we define a reflex as a given observed correlation or as a statistical treatment of observed correlations, the questions are meaningless, for they ignore the process of analysis implied in the definition. A reflex, that is to say, has no scientific meaning apart from its definition in terms of such experimental operations as we have examined, and, so defined, it cannot be the subject of questions of this sort.

There is a certain practical advantage, it is true, in regarding a reflex as a unitary mechanism—an advantage, as Mach might have said, that may have given rise to the practice. It is only when we misconstrue a purely practical device and take it to be an integral part of our definition that the possibility of theoretical misunderstanding arises. Our sample questions deal necessarily with the reflex defined in terms that we have seen to be well beyond any observational justification. As Poincaré has said of a similar issue, "*ces questions*

ne sont pas seulement insolubles, elles sont illusoires et depourvues de sens" (18, p. 192). A common mistake in the present case has been to suppose that, because an answer is lacking, the principle of the reflex is somehow impeached. As we have repeatedly noted, the validity of the reflex as a scientific concept is not here in question. The reflex remains, as it has always been, an observed correlation of stimulus and response.

VII

It remains for us to consider how a reflex as a correlation is dealt with experimentally. The first step, as we have seen, is the isolation of a response and the identification of its correlated stimulus. In practice, the demonstration of the correlation is usually left at an elementary level. It is based upon the appearance of the two events together and their failure to appear separately. As an experimental datum of this sort, a reflex may be given the expression

$$R = f\ (S) \qquad [1]$$

where R is a response and S a stimulus. Theoretically, the exact nature of the function is determinable, although for any present purpose corresponding values of S and R are obtainable by observation only. Choosing convenient measures of both stimulus and response, we may vary the strength of S and observe variations in the strength of R. This is common practice, although very little has been done toward determining how a given R varies with its corresponding S. One characteristic of the relationship is the threshold: for values below a given value of S, $R = 0$. There are also temporal aspects of the function, which have been investigated under the headings of latency and after-discharge.

Threshold, latency, after-discharge, and the order of variation of S and R are thus descriptions of the correlation that we call a reflex. They may be investigated with only one elicitation of the reflex or, at most, with a single set of corresponding values of S and R. There is a second field of investigation, however, which is concerned with variations in any aspect of a correlation, as they may appear in the comparison of successive elicitations. If, for example, we select a value of S and repeat the elicitation of the reflex at a given rate, we shall observe a progressive decrease in the value of R. Or, again, if the interval between two successive elicitations be made brief enough, the second R may be of greatly reduced magnitude or wholly lacking. Here are significant variations in the value of the terms in

Equation [1]. They do not challenge the necessity of the relationship expressed therein (as they might well do if they were less orderly), but they do require that, in the description of a reflex, account be taken of *third variables.* We may indicate the required change by rewriting our equation as

$$R = f(S, A) \qquad [2]$$

where A is a variable designed to account for any given observed change in the value of R.

As it appears in such an experiment, A is properly either time or the number of elicitations at a given rate. The inference is commonly made that it represents a factor of another sort, which varies with time or the number of elicitations in the same way. In the first example noted above the phenomenon has been called reflex fatigue, which is regarded as a synaptic change—as the exhaustion of a substance or state, or as an increase in resistance, according to one's preference in synaptic theory. But in the description of behavior, where we are only secondarily interested in these physiological inferences, reflex fatigue is nothing more than an orderly change in some measured aspect of a given correlation. A law describing the course of that change, where the independent variable is time or the number of elicitations or some other condition of the experiment, is peculiarly a law of behavior. It may become a law of the synapse, by virtue of certain physiological inferences, but it has by that time passed beyond the scope of the description of behavior.

Nevertheless, if we are to follow current usage, a definition of reflex fatigue as an observed variation in *one* aspect of a correlation is too narrow, for we know from observation that, when such a change has taken place, the other aspects of the correlation have also changed. If we have observed, for example, a change in the ratio of a particular R and S, we may expect to find all other ratios, as well as the threshold, latency, and after-discharge of the reflex, likewise changed. It is usual, therefore, to regard the particular change that we chance to observe as a sample of a greater process. Occasionally, where a change in one aspect of a correlation is alone important (as in summation, which is chiefly a matter of threshold), the characteristic may possibly be defined in terms of a single change. But such a characteristic as reflex fatigue, or the refractory phase, or facilitation, is by intention a description of a group of concurrent changes.

If we are to speak in terms of these group changes, it is almost necessary to have a term describing the *state* of a correlation at any given time with respect to all its aspects. The physiologist, of course, may use the synapse for this purpose. When he has once described reflex fatigue as the exhaustion of a synaptic substance, for example, he may attribute a change in *any* aspect of a correlation to that exhaustion. Although he may observe and measure at one time a change in after-discharge and at another a change in the magnitude of R, he may reasonably consider himself to be dealing with the same process in both cases. Fortunately, there is also a term serving the same purpose at the level of behavior. If, in a given reflex, the threshold is low, the latency short, the after-discharge prolonged, and the ratio R/S large, the reflex is ordinarily said to be strong. If, on the other hand, the threshold is high, the latency long, the after-discharge short, and the ratio R/S small, the reflex is said to be weak. An attribute of *strength* is imputed to the reflex. The strength of the response, of course, is not meant; a weak response may indicate a strong reflex, if it be elicitable with a very weak stimulus.

"Reflex strength" expresses in a very general way the state of a given correlation at a given time in respect of many of its characteristics. It is a useful term, for it permits us to deal with reflex fatigue, for example, as a *change in reflex strength,* without stopping to specify the particular changes that compose it. Nevertheless, its usefulness does not extend beyond this qualitative level. The concept is subject to a major objection, which holds as well for the parallel use of the synaptic state. We do not know, since it has never been determined, whether the changes that compose such a characteristic as reflex fatigue all proceed at the same rate. If the threshold, let us say, and the magnitude of R do not vary in precisely the same way, we are not justified in taking either as a measure of a supposed common variable, nor, indeed, in continuing to regard reflex fatigue as a unitary process.

The study of the reflex, then, leads to the formulation of two kinds of law. The first are laws describing correlations of stimulus and response. A reflex, as we have defined it, is itself a law, and of this sort. It has a considerable generality in spite of the specificity of its terms, but it must be supplemented by other laws describing the exact conditions of a correlation. Secondly, there are laws describing changes in any aspect of these primary relationships as functions of third variables, where the third variable in any given

case is a condition of the experiment. These secondary laws may be dealt with in groups, according as they involve the same experimental third variable, and they may be spoken of, for convenience, as describing changes in reflex strength. In the behavior of intact organisms the apparent variability of specific stimulus-response relationships emphasizes the importance of laws of the second sort. Conditioning, "emotion," and "drive," so far as they concern behavior, are essentially as changes in reflex strength, and their quantitative investigation may be expected to lead to the determination of laws describing the course of such changes, i.e., to laws of the second sort.[12]

It is difficult to discover any aspect of the behavior of organisms that may not be described with a law of one or the other of these forms. From the point of view of scientific method, at least, the description of behavior is adequately embraced by the principle of the reflex.

Summary

The present anlysis of the reflex as a concept in the description of behavior follows the method first formulated with respect to scientific concepts by Mach and Poincaré. It examines the source of the historical definition and points out the incidental nature of most of its criteria. Eventually, it offers an alternative definition and considers in detail some of the questions that arise from the nature of the concept so defined.

I. Descartes "discovered the stimulus" and designed a mechanism which could account for animal movement upon the basis of the appropriate release of stored energy. But he was interested less in describing the action of the nervous system than in supporting metaphysical contentions of the automaticity of animals. He advanced the stimulus as a substitute for soul, but only within a field which omitted the greater part of the activity of man.

II. The notion of the reflex developed, independently of Descartes, from the investigation of "irritability." The action of a stimulus was implicit in the concept of irritability, which also assigned an autonomy of function to the parts of an organism. The concept of the reflex arose quite naturally when a stimulus and its related response were to be spatially distinguished. Robert Whytt made the first historically effective observations.

[12]The report of an experimental study of "hunger drive" from this point of view is in preparation.

III. It remained for Marshall Hall to clear the concept of psychical counterparts. This he did by setting up a distinction between reflex and voluntary action, which resulted eventually in the unfortunate historical definition of the reflex as a form of movement unconscious, involuntary, and unlearned. Volition, in Hall's sense, was essentially the hypothetical antecedent of movement for which no corresponding stimulus could be observed, a definition which served to identify the reflex with scientific necessity and volition with unpredictability.

IV. The history of the reflex has known only one positive characteristic by which the concept may be defined: the observed correlation of two events, a stimulus and a response. The negative characteristics, on the other hand, which describe the reflex as involuntary, unconscious, and unlearned, have proceeded from unscientific presuppositions concerning the behavior of organisms. The reflex is tentatively defined herein as an observed correlation of stimulus and response.

V. Reflex physiology undertakes to describe the events which intervene between a stimulus and a response. The physiological usage does not question the definition of a reflex as a correlation, for the synapse is only a conceptual expression for the "reduced" characteristics of a given correlation.

VI. The essense of the description of behavior is held to be the determination of functional laws describing the relationship between the forces acting upon, and the movement of, a given system. The reflex is, by definition, the precise instrument for this description. Its analytical nature is discussed, and existing methods of analysis are examined. Current objections to analysis are held to have no scientific meaning.

VII. The experimental study of the reflex may be divided into two parts. There is, first, the investigation of the characteristics of a correlation—latency, threshold, after-discharge, and the order of variation of *S* and *R*. Secondly, there is the investigation of variations in these characteristics as functions of third variables. The notion of reflex strength is useful in dealing with this second group. The question of third variables is of extreme importance in the description of the behavior of intact organisms.

From the point of view of scientific method, any law describing the behavior of organisms must be reducible to one of the forms herein discussed. The description of behavior, that is to say, is adequately embraced by the principle of the reflex.

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LE CONCEPT DU RÉFLEXE DANS LA DESCRIPTION DU COMPORTEMENT

(Résumé)

Un examen de l'histoire du réflexe indique que la définition historique n'a jamais été tout à fait justifiée et qu'elle comporte des interprétations occasionnelles (décrivant le réflexe comme *in*conscient, *in*volontaire, *non* appris et ainsi de suite) lesquelles remontent à des présuppositions non scientifiques concernant le comportement des organismes. On offre une définition alternative, basée sur les seules évidences positives fournies par les données, où le réflexe est défini comme une corrélation observée de deux événements, un stimulus et une résponse. L'emploi phyisologique du mot et la méthode physiologique sont examinés et l'on trouve qu'ils ne nuisent pas à cette interprétation. Ensuite on discute certains aspects du rapport entre le réflexe et la description du comportement. On considère que l'analyse est nécessaire à cette description, et on examine les méthodes de l'analyse. On considère certaines questions actuelles remontant à l'emploi de l'analyse et les trouve sans signification. On décrit brièvement le rapport entre son processus expérimental et le réflexe comme ici défini, et on définit le concept "force du réflexe" et appuie sur son importance. On considère que la description du comportement se compose de la détermination de lois fonctionnelles comme celles ici présentées et ainsi qu'elle est incluse presque entièrement dans le principe du réflexe.

SKINNER

DER BEGRIFF DES REFLEXES IN DER BESCHREIBUNG DES VERHALTENS

(Referat)

Ein Ueberblick über die Geschichte des Reflexes weist darauf hin, dass die traditionelle Definition nie vollkommen berechtigt worden ist, und dass damit beiläufige Deutungen verbunden worden sind, (Beschreibung des Reflexes als *un*bewusst, *un*willkürlich, *un*gelehrt, usw.), welche ihren Ursprung haben in unwissenschaftlichen Voraussetzungen über das Verhalten der Organismen. Es wird eine alternative Definition dargeboten, worin der Reflex als eine beobachtete Korrelation zwischen zwei Begebenheiten,—namentlich zwischen Reiz und Reaktion,—erklärt wird. Der Gebrauch des Wortes 'Reflex' in physiologischem Sinne und die physiologische Methode werden untersucht, und man findet, dass sie diese Deutung nicht entkräften. Es werden dann gewisse Seiten der Beziehung des Reflexes zu der Beschreibung des Verhaltens (behavior) besprochen. Die Analyse wird als zu dieser Beschreibung nötig betrachtet, und es werden analytische Methoden besprochen. Gewisse herrschende Fragen, welche im Gebrauch der Analyse ihren Ursprung haben, werden erwägt und als ohne Bedeutung betrachtet. Die Beziehung unseres experimentellen Verfahrens zu dem Reflex wie er hier definiert worden ist, wird kurz erörtert. Der Begriff der 'Reflexenstärke' (reflex strength) wird erörtert, und die Wichtigkeit dieses Begriffes wird betont. Es wird behauptet, dass die Beschreibung des Verhaltens aus der Bestimmung funktioneller Gesetze von der Art des hier dargebotenen bestehe, und dass sie also fast vollständig in dem Prinzip des Reflexes eingeschlossen sei.

SKINNER

THREE GENERATIONS OF COLOR-BLINDNESS*

From the Department of Philosophy and Psychology of the University of Wyoming

JUNE E. DOWNEY

The ideal procedure in presenting a case of familial color-blindness would involve a testing of the color sensitivity of every member of the family concerned. Such an ideal procedure is rarely a practical possibility—an unfortunate limitation since the slurring of transitional cases of color defect has undoubtedly led to an undue simplification of data, which, in turn, has encouraged some arbitrariness in theories of color vision, as well as in the tracing of the inheritance of color defects. The most that can be achieved under usual conditions is a statement of the form of color-blindness occurring in the defective members as determined by some one or more forms of accepted color-blind test.

Even the application of the latter tests and an attempted interpretation of them can have only limited value, since, as is recognized at the present time, the science of color vision is in a well-nigh hopeless condition, with observation and theory so entangled that only the trained and unbiased specialist has any chance of making comprehensive and accurate reports on cases of defective color vision. The writer is not such a specialist. Furthermore, the observations that could be made were limited by the amount of time that the subjects put at her disposal. There were, moreover, other serious limitations in the way of laboratory and library facilities.

The family record I am to report thus inadequately includes seven cases of color-blindness distributed in three generations as shown in Figure 1. The color-blind in this chart will be referred to by letter, as shown in the genealogical chart. They include O in Generation I; C, S, and K in Generation II, and R, Ki, and T in Generation III. The year of birth is given for each in the table.

This family record can be interpreted in terms of a frequently quoted theory of the transmission of color-blindness, namely, that it

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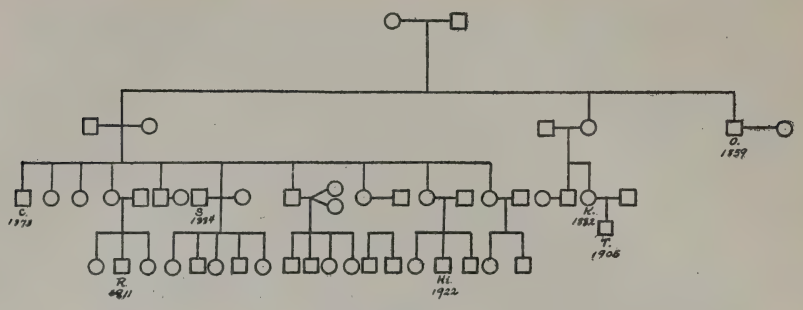

FIGURE 1

is inherited in accordance with Mendelian expectation, except that it is sex-linked, recessive in women and dominant in men.

The presumption under this theory would be that the mother of O-I was a carrier of the defect, although she herself was excessively sensitive to color. Nothing is known of the color sensitivity of her father and brothers, nor of the color sensitivity of O's father. O's sister, E, is a carrier of color-blindness and has transmitted it to one-half of her sons, and the defect appears in the sons of at least two of her daughters.

The case of K-II, daughter of O's other sister, is less clear-cut. Here we have a color-blind woman and, *under the theory,* must assume not only that her mother was a carrier but that her father was actually color-blind.[1] There is no evidence that the latter assumption is a fact. An effort has been made to inquire into the color vision in his family but without result. K's paternal aunt states that no color defect has been known in the family. But, in the absence of actual tests, color-blindness frequently goes undiscovered. K's one brother has normal vision. Recently he passed the Ishihara test without error. He himself reports that he has some difficulty distinguishing between blues and greens.

K's color defect appears to be not quite comparable to that in the other branch of the family. The same statement holds with regard to T-III, son of K. Tested with the Holmgren worsteds, he does not reveal his defect. This fact led, some years ago, to a correspondence with Bateson, the geneticist, since, according to Mendelian expectation, all sons of color-blind women should also be color-blind.

[1]It is, of course, possible that K. inherits her color disability wholly from the paternal side. The discussion above is motivated by the theory cited.

Bateson in 1913 quotes this case as a possible exception to the general rule of color-blindness in the sons of color-blind women.[2]

Later tests of T with more satisfactory material have given evi-

[2]"Here it must suffice to say that the most obvious test of the nature of the inheritance is provided by the families of color-blind women. According to the scheme the simplest expectation is that *all sons* of such women should be color-blind. Up to the present time we have records of seven color-blind women only who had sons. In all they had 17 sons who lived to be tested, and all were found to be color-blind. We may therefore rest assured that the scheme provides at least a substantial part of the truth and that color-blindness is a condition produced by the addition of a dominant factor" (1, p. 173).

A footnote reads as follows: "By the kindness of Miss J. E. Downey of Wyoming University I have since been informed of a case which is probably a real exception—a woman with defective color-sense having a child with normal color vision" (1, p. 224).

I give also a portion of a letter written to Bateson by myself in 1909 relative to the testing of K and her son T.

"There can be no doubt that K is color-blind, although her case is not wholly analogous to that of her uncle and cousins. She has one son, aged four years, nine months [mental age probably considerably higher, since T is of superior intelligence], whom I recently tested with the Holmgren worsteds. He is not color-blind. The details of the test follow:

"The child was first tested June 30, 1909, aged four years, four months. He matched the worsteds very confidently. In the case of the rose-purple or magenta he included two or three of the blue confusion colors, otherwise he showed no defect of color vision.

"July 1, the test was repeated with better illumination. No confusion of colors was noticed. The boy absolutely refused, however, to use the rose-purple as a sample. It was thrown aside and a blue skein given him. He matched this correctly without grouping with it any of the violet or purple tints, which he continued to ignore throughout the test. It is possible that his disinclination to match this color was due to the fact that he could not name it as it was noticed that he named the colors to himself as he sorted them. As his mother calls all purples, violets, and magentas 'blue', he possibly had no word for this color. At least he did not use such a word, although he named correctly reds, blues, greens, and browns. The child's accuracy in sorting the worsteds was in great contrast to the confusion shown by his mother who was tested immediately afterwards. She introduced confusion colors in matching both green and rose-purple.

"October 15, the child was again tested, aged four years, eight months. Since the time of the first test he had had several weeks' training in kindergarten. This third test was absolutely conclusive. There was no evidence of color confusion. The child matched the colors confidently and named them accurately, although he still had no name for the magenta or rose-purple. He was tested first with this color to avoid the fatigue shown toward the close of the other tests. He accepted the skein and matched it perfectly. He placed separately several groups of colors as reds, greens, blues, purples and magentas, browns and yellows. The boy was also given at this time a Bradley color top with discs of the spectral colors. When asked to name the violet disc, he called it, after hesitation, 'blue.' On this occasion he was given its name and thereafter he used the word without hesitation and correctly."

dence that he has a color defect. It is probable that, had these more delicate tests been used when T was tested as a child, the defect would have been revealed then. It was, however, the thought of the writer in 1909 that T should be retested after puberty, with the rather wild speculation that, since color-blindness is somehow tied up with the chemistry of sex, color-blindness might develop with sex maturity.[3] Under the circumstances, this speculation has little value, but it is curious that, as a child, T passed the Holmgren test more confidently and speedily than now seems possible, although he may have developed caution because of doubts cast upon the normalcy of his color vision. He is undoubtedly less color-blind than his mother, in fact, least color-blind of the seven subjects.

It is, however, unnecessary to describe facts in terms of a theory. Says J. Drever:

> "The hereditary character of color-blindness was early recognized. The actual facts, however, appear to be somewhat complex. We have come across several cases where brother and sister both showed color-weakness, but on being tested the brother was found to be definitely color-blind, while the sister could only be characterized as color-weak. It is known also that color-blindness may occur in the males of one generation, be latent in the next generation, especially in a female generation, and occur once more in males of the succeeding generation. It has been said that its inheritance passes by the female line. Whether the data available are sufficient to establish this or not, it is certain that the color-blindness is for the most part latent in the female, though possibly indicating its latency by color-weakness" (2, p. xxvi).

Because of this complexity of factual material, it is, as we have said before, unfortunate not to have tests on all members of the present family in the hope of discovering cases of color-weakness as well as of color-blindness. All members of Generation II have been given some simple form of test for color-blindness; and all in Generation III where there has been any suspicion of a color defect. The four oldest children of S all pass the Ishihara Test. Such summary

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[3]Detailed studies of the development of the color sense in children with defective color vision may make a contribution to our understanding of the relation of sex to color vision. These studies will necessarily concern themselves with individuals. Citation of percentage of color defects for groups of children of various ages will not be sufficient.

handling is not, however, sufficient to throw out cases of color-weakness.

There is much eye trouble—myopia—in the family, but largely in members other than those afflicted with color defect. The writer of this paper (a member of Generation II) is hypersensitive to light and to simultaneous and successive contrast, and has a restricted zone for green vision as shown by perimeter measurements. Some peculiar confusions of yellow and pink occur under artificial illumination, confusions that, I believe, are not uncommon. Recently, under artificial illumination, a brightly-violet, woolly toy dog was definitely seen as golden-brown, the yellow light neutralizing the blue of the violet. Certain authorities have stated that color-blind (or color-weak?) individuals are particularly sensitive to simultaneous and successive contrast—a point which I have been unable to confirm.

In the discussion that follows tables will be given that permit a comparison of the color reactions of the seven color defectives. All have been tested with the Ishihara plates under standard conditions. All have also had one or more tests with the Holmgren wools, and several have been given, in addition, the Nagel, Stilling, and other tests. In addition to reports on these standard tests, a few comments will be made on individual cases and also on the problems involved in color terminology and in the testing of the youngest member of the group. At the close of the article is given a report by one subject, O, on his negative after-images.

By the Ishihara plates all of the seven subjects would be described as color-blind. Plates 1 to 11 classify them as red-green blind, according to Ishihara's terminology; Plates 12 and 13, as completely green-blind. There was very little difference in the records. What little there was may be seen at a glance in Table 1; Plate 2, O fails to report any numeral; C reports 5 instead of the more usual 3; S reports 3 or 8. Plate 5, Ki reports 2 instead of the usual 21. Plate 12, O reports 21 instead of 2.

When the charts are placed ten feet distant, as suggested by Miles to test foveal vision, the results are somewhat surprising. But distance, it must be recalled, changes intensity as well as the visual angle. The summary will be found in Table 2. T varies only slightly from his former record; K and Ki more so; O and S fail to see any numerals whatever; while C and R give very peculiar results, failing now to see any numeral on Plates 2, 3, 4, 5, 10, and 11, and getting the correct or almost correct numerals on Plates 6, 7, 8, 9, 12, and

TABLE 1
ISHIHARA COLOR-BLINDNESS TEST, FIFTH EDITION

Plate→	1	2	3	4	5	6	7	8	9	10	11	12	13	Point Score*
Normal→	12	8	6	5	74	2	6	5	7	—	—	26	42	
O (M)	12	—	5	2	21	—	—	—	—	5	2	21	4	2
C (M)	12	5	8	2	21	—	—	—	—	5	2	2	4	3
S (M)	12	3	(8) 5	2	21	—	—	—	—	5	2	2	4	2 or 3
K (F)	12	3	5	2	21	—	—	—	—	5	2	2	4	2
R (M)	12	3	5	2	21	—	—	—	—	5	2	2	4	2
Ki (M)	12	3	5	2	2	—	—	—	—	5	2	2	4	2
T (M)	12	3	5	2	21	—	—	—	—	5	2	2	4	2

*For description of test and method of giving a point score, see Miles (3). My subjects read the plates at the distance suggested by Ishihara and not at the four feet used by Miles.

TABLE 2
ISHIHARA COLOR-BLINDNESS TEST, FIFTH EDITION
PLATES AT TEN FEET DISTANCE

Plate→	1	2	3	4	5	6	7	8	9	10	11	12	13	Point Score
Normal→	12	8	6	5	74	2	6	5	7	—	—	26	42	
O (M)*	—	—	—	—	—	—	—	—	—	—	—	—	—	0
C (M)	12	—	—	—	—	2	6	5	7	—	—	25	42	7
S (M)†	—	—	—	—	—	—	—	—	—	—	—	—	—	0
K (F)	12	—	2	2	27	—	—	—	—	2	—	—	4	1
R (M)	12	—	—	—	—	2	6	8	7	—	—	26	42	6
Ki (M)	12	2	2	3	5	—	2	—	4	5	3	1	4	1
T (M)	12	3	2	2	21	5(?)	—	7	—	4	2	2	4	2

*Because of cataracts in both eyes, it was very hard for O to see anything at a distance, which may account for the failure to see any numerals at ten feet distance.
†S probably needs to be fitted for glasses.

13. It is only with very bright illumination that C reads the numbers on Plates 6, 7, 8, 9 at ten feet distance. He reports that he sees no unusual color under such circumstances and describes "7" of Plate 9 as a light blue (instead of green) on red. It was possible once to get this same "7" at reading distance with bright electric illumination. Degree of illumination is most important. C, when first tested with the Ishihara plates in the late afternoon, failed to perceive any numerals whatever, although he gave the usual red-green blind results when illumination was bright. R reports that "6" on Plate 7 appears at ten feet as the same color as the background but

lighter. (It is actually pink on a gray and green background). On Plates 12 and 13, "6" and "2" came in at about seven feet distance; they seem to become lighter than the background.[4]

Besides the Ishihara test, the seven subjects have each been given the Holmgren worsted test. Here the results were not uniform. The Ishihara test classifies all of the seven as completely green-blind and incompletely red-blind. The Holmgren and other tests would seem to reveal different degrees of red-blindness and possibly of green-blindness among the seven. The members of the older and of the younger group may be discussed separately.

T, as was stated earlier in the paper, is able to pass the Holmgren test with slight difficulty in matching the purplish-pink skein. There is sometimes a little hesitation concerning inclusion of browns with the greens, a hesitation which can be made very evident (with some final selection of browns) by giving the test with reduced illumination. T is aware of this difficulty. He uses the word "green" and "brown" to all appearances correctly. He also describes a violet as a cross between red and blue. Fifteen skeins of Holmgren worsteds were named correctly by him. The light violet was called "orchid," which color he said he understood to be a mixture of blue and pink. Magenta (dark purple-red) he called "red" or maybe "purple"; slate was called "brown." T is very conservative in inclusion of colors; when in doubt he rejects a color. He uses "green" as an inclusive term and, apparently, never employs the term "gray."

R had more difficulty than T with the Holmgren test but less than the other five subjects. He was very slow in seeing any green skeins and included browns and slates among his matches. He stated, "Green, tan, and brown are the ones I confuse." The red skein was matched quickly and confidently, with slight hesitation over light tan and light green, which were finally rejected. He called the purplish-pink skein lavender and had difficulty with it. He is aware of difficulty with violet and blue. One, he says, is dark, one light, but he doesn't know which is which. R gave many more errors in naming the fifteen skeins utilized than did T. He called a vivid green skein red, used brown and red interchangeably, and confused blue and purple. Slate was called "brown"; and magenta "scarlet."

Ki includes many nondescript neutrals and light browns among his

[4]The writer has the interesting experience of losing "6" on Plate 12 and "2" on Plate 13 at about 8 feet distance. This gives an excellent idea of the green-blind reaction to these plates.

green matches. He matches this skein mournfully and without confidence.[5] He includes some dark browns with the red skeins; and matches the purplish-pink skein with blues, violets, and included one green. Many errors occur in naming the fifteen skeins, vivid green is called a "kind of orange"; brown called "green"; light green, "gray"; slate, "green"; blue, "purple." His defect is, apparently, much more serious than that of T or R.

We turn now to the group of older subjects. O, C, and S were given the Holmgren test at the University of Wyoming in the 90's by the late E. E. Slosson and classified as green-blind, green-blindness being interpreted for them in terms of the Young-Helmholtz color theory, with its very dubious deductions as to what colors the green-

[5]Observation concerning the effect upon a child's personality and his vocabulary of a pronounced color defect should be made in preschool studies. S, as a small child, developed a great dislike for kindergarten because of his teacher's attitude toward his inability to do color work. R, while in the primary grades, was in difficulties because his teacher refused to believe that he had a color disability.

Ki, aged eight years, the youngest subject of the present study, is greatly distressed by the notion that he may be color-blind. He is inclined to attribute his defect to the fact that he has not received careful training—an evidence of rationalization on his part. Up to the present he has refused to accept the author's verdict, partly, perhaps, because of his ambition to go to West Point—one that color-blindness would interfere with, as he is aware. His attitude is evidenced by a remark made when he was given the Ishihara Test. After he had read the numerals on Plates X, XI, XII, and XIII, he said, "I would think any one who couldn't see those numbers goofy." Then he turned thoughtfully back to the plates on which he had seen no numerals and asked, "Is that what they think of me?"

It was his lack of interest in color words that first raised in early childhood the question of his being color-blind. This lack of interest was very noticeable, since it was in great contrast to his general facility with words. I believe that careful observation would have shown that this failure of interest carried over to certain objects in his environment which were characterized chiefly by their color. Certainly, his behavior as an infant and small child was in striking contrast to that of his brother, Ri, five years younger, who shows an intense interest, for example, in the color of the clothes worn by those around him. For instance, a pair of light green slippers of a cousin's aroused Ri's great admiration before the age of three. He never failed to note when they were worn and never confused them with similar slippers tan or gray in color. Tested with the Holmgren worsteds at the age of three years, three months, Ri makes quick, darting movements in matching the green yarns, very unlike the slow, hesitant movements of Ki. He has no difficulty in picking them out of the mass of skeins.

It is, of course, possible to develop so great an interest in the problem of color-blindness itself as to compensate for the social disability the latter entails. This is evident in the case of the eminent authorities on color vision who are color-blind. It is also evident among the present subjects in O's case.

TABLE 3

STILLING'S PSEUDO-ISOCHROMATIC PLATES

Sixteenth Edition, edited by Dr. E. Hertel

	I		II				III		IV		V				VI				VII		VIII		IX		X		XI		XII	Point
	1	2	1	2	3	4	1	2	1	2	1	2	3	4	1	2	3	4	1	2	1	2	1	2	1	2	1	2	1	Score
C	¼	—	—	—	—	+	½	—	¼	—	—	—	—	—	—	—	—	—	—	—	—	½		+	¾	+	+	+	+	8¼
K	+	—	¼	—	—	¼	+	—	—	—	—	—	—	—	¼	—	—	—	—	—	—	½	½	+	¾	+	+	+	¾	9¼
R	½	¼	¼	—	—	+	+	¾	⅜	—	—	—	—	—	—	—	—	—	—	—	—	+	+	+	+	+	+	+	+	12⅛
Ki*	—	—	¼	¼	¼	+	+	—	—	—	¼	—	—	—	—	—	—	—	—	—	—	—	¼	+	¾	+	+	+	¾	8¾
T	¾	—	¼	—	—	+	+	¾	¾	¼	—	—	—	—	—	—	—	¾	—	—	—	+	+	+	½	+	+	+	+	13

*Ki was not used to "4" as made in these tables. He did not recognize it as a four, although once, at least (II, 4), he traced it with his finger.

TABLE 4

NAGEL CARD TEST, SIXTH EDITION, SECTION A*

1. *Red or Reddish Spots*

Correct	1	2	3	—	—	—	7	8	—	10	—	—	—	14	15	16
C		2				6					11	12				
S						6					11	12				
K	1	2	3				7	8		10				14	15	16
T	1	2	3				7	8		10				14	15	16

2. *Red Spots only*

Correct			3													
C												12				
S	None															
K			3												15	
T			3												15	

3. *Green Spots only*

Correct					5											
C	1	2	3	4	5	6	7	8	9	10	11		13	14	15	16
S	None															
K				4	5	6			9		11	12	13			
T				4	5				9				13			

4. *Gray Spots only*

Correct									9							
C			3	4			7		9	10						
S			3				7		9						15	
K									9							
T	None															

*For description of the Nagel Card Test, see (2, pp. 147 ff.).

blind man sees. O's matches for the green sample include light-browns; for the red, dark browns; and for the purplish-pink, purples, blues, and green-blue.

O, who was intensely interested in color-blindness for many years and who was constantly comparing his experiences with those of others, satisfied himself that the color vision of C and S was thoroughly like his own. The three agreed definitely in their matches. The reactions of K, however, baffled her uncle. She evidently failed to check up with his anticipations. He was inclined to think that she was careless and indifferent in taking the tests.[6] K includes,

[6]O's reaction to this case is interesting in the light of the fact that a great deal of the work on color-blindness has been done by men who were themselves color-blind. Is it possible that this fact accounts for failures to reckon with the great variety of individual variation?

among matches with the green sample, drabs, light to dark browns inclusive, and a few blues; finds few matches for the red; and includes almost the entire series of blues and purples as well as a greenish blue in matching the purplish-pink skein. It is possible that she is less discriminating than some of the other observers and when in doubt simply includes a color for good measure. She is also less consistent in her matches than is O or T. Apparently her color defect is much more serious than that of T, her son.

The other tests that have been given confirm the results of the two reported in detail above, although it has not been possible to get records for all the subjects. Five of the subjects, C, K, R, Ki, and T were tested with the Stilling Pseudo-Isochromatic Plates, Sixteenth Edition. The results are summarized in Table 3. Again T and R make a better showing by a point score than do the others. The sixth edition of the Nagel Card Test was used with C, S, K, and T. The results are summarized in Table 4. The interesting thing here is that there were certain points of similarity in the records of C and S (brothers) and of K and T (mother and son). On Question 1 of this test, namely, "On which cards are there red or reddish spots?" C and S make errors which Nagel states are "almost complete evidence of color-blindness." K and T select these cards without an error. It is possible that K and T would be classed as "anomalous trichomates" by the Nagel test. Other results in T's case point in this same direction; K's records are more dubious.

Color naming has been taboo as a method of studying color-blindness, but Collins has shown that it may be used profitably since intelligent color-blind individuals do not use color words at random but work out a consistent system. Quite likely one can gain more insight into the experieces of a color-blind individual through a study of his color-naming scheme than by making theoretical deductions from a favored color theory.

Since all of the present subjects are obviously defective in green vision, it is worthwhile noting to what colors they apply the term "green" and, in turn, what names they give to green. It is also important to check on their use of the word "red" and its variants and the color terms applied to various reds, since the degree to which any or all of them see red is a crucial matter in determining the extent to which they suffer from a comparable defect. Information concerning color terminology is, however, satisfactory only for the four sub-

jects C, R, Ki, and T, who not only named the chosen Holmgren skeins but also described the dots on the Stilling plates.

T applies the word green to various tints and shades of green and also to yellowish green, to reddish and dark browns, and to neutral colors including grays of all tints and shades. Once he used the term for black and for a greenish yellow. He had not, prior to the experiments, used the word gray. When questioned about this, he was evidently surprised and answered "You don't call gray a color, do you? It is a word applied to clothing, to hats, suits of clothes, and the like! Mixed goods"[7]—a remark which shows the curious possibilities in matching words and meaning under the handicap of a sensory disability. The gray of the Witmer color contrast plates he calls "steel" or "silver." The word "red" he uses apparently as does the normal-eyed. It is only with a very unsaturated pink that he has trouble. He confuses this pink with a tan, although he refuses to name it. On the Nagel test he once chose a card with two grays for a card containing nothing but reds. A light pink worsted skein was described as "maybe a little red, white or soiled white." I have never found him applying the word "green" to red nor "red" to green. The Ishihara test tripped him, but it is difficult to believe that his difficulty with red is in any way comparable to that with green. I should be inclined to believe that his only difficulty is with unsaturated reds.

R confused the words "red" and "green" hopelessly, but apparently not without system. He uses light green to refer to red and dark green to a dark red or reddish-brown. Greens are spoken of as "greens," and, while reds are often correctly named, pinks are called tans as is also a greenish yellow. He uses "crimson" and "blue" as synonymous. In spite of these errors, R is apparently much less confused with reference to red than is C.

C uses green for dark red, dark brown, slates, and grays of all kinds. He remarks that green is only a sort of gray. His bad confusion of red and green is shown by his selection of fifteen cards in the Nagel test in answer to the question, "On which cards are there only green spots?" Many of the cards he chooses have vivid red spots on them. He finds it very irritating to attempt to name the

[7]This parallels a situation found in a tonally deaf individual with whom the author worked. It developed that the word "pitch" very definitely meant for her variations in intensity. High pitch signified soft tones; low pitch, loud ones.

dots on the Stilling plates. He described them as a conglomerate mass of all sorts of colors except blue and yellow. The dots changed color at each movement or flicker of changed illumination. There are some observations of confusion of red with black, which would indicate a shortening of the red end of the spectrum. I have been inclined to believe that C's vision is definitely a blue-yellow vision, and I have had many arguments concerning this with C, who insists that he sees red as qualitatively different from yellow. His arguments, however, are not convincing. He instances his bewilderment at the difference commonly cited in the color of oranges and lemons. He says he can see little difference between them "except for some California oranges which are not yellow at all but red." This example leads one to suspect that C's red is really a yellow. In the Stilling plates the orange dots are called light red; greenish yellow dots and tan are also called red. Possibly the fact that he was early labeled green-blind in the Helmholtzian sense has set him in his opinion that he does see "red." The outcome of the Ishihara test would, however, agree with C's own opinion of his color defect, since it classifies him as green-red blind, completely green- but incompletely red-blind.

Ki uses green and gray as interchangeable expressions. He applies the word green to light browns also. It was very difficult to get him to attempt to name the dots on the Stilling plates. He largely described them as brown and orange. His color vocabulary is distinctly limited, but I am inclined to believe that both his green and his red vision are seriously defective.

As regards the other three members of the group, notes on their color terminology are very limited, since when they were tested great care was taken to avoid the use of words. Judging from S's reaction to Section B of the Nagel test, it would appear that he uses the word gray rather than green as the inclusive term. It may be somewhat a matter of chance as to whether the word gray or green will be applied by the green-blind to a great variety of neutral shades. It is quite possible that it may become habitual for a green-blind person when in doubt to respond either with the word "green," or, as seems to be the case for S, with the word "gray." S's difficulty with red is shown in both the Ishihara and the Nagel tests. Judging from various experiences, such as failure to see signal lights, his color vision is quite as defective as C's.

My notes on K's color vocabulary are meagre. There is, possi-

bly, something peculiar about her color-vision which has not been identified by the tests, although the Ishihara test classifies her as green-blind with the other members of the group. She apparently loses color at a very short distance from the stimulus. This was shown by an experiment in which K advanced toward bits of Bradley's colored papers (2 cm. x 3 cm.) from a distance varying from 15 to 20 feet. She reported on the colors as she moved toward them. Bright red, purple, violet, rose, and magenta were all reported as green until within 2 feet, when she saw them respectively as red, blue, red, red, and an unnameable color. Blue was the only color which was seen as itself at 15 feet distance. It is quite likely, of course, that the green reported above is really a gray, although K insists that it has plenty of color and is not neutral. With the Scripture Color Sense Tester, K describes the transparent glass when seen through either of the smoked windows as green or pink. Sometimes it is called the one, sometimes the other, sometimes either one or the other. She always wished to bring the glass close up to her eye before giving judgment. When the glass was held far off and then brought up close to the eye, she would frequently speak of a green changing to red. K did not understand what was meant by a neutral zone and, when shown Prang's Prismatic Chart, insisted that the whole was colored. Blues and purples, rose and magenta, are hopelessly confused. I do not, however, feel at all confident as to how K's case should be described.

O's color vocabulary is by far the largest and most varied of the present subjects. This is due, no doubt, to his very great interest in color-blindness as a practical and theoretical problem, his fine sensory discrimination and his possession of an unusually large vocabulary in general. Table 5 gives some notion of the terms used by him. He speaks of red, ruby, crimson, terra-cotta, rose; of sulphur, canary yellow; of purple, plum, violet-blue, lavender-gray, and the like. It is not, of course, to be inferred that he sees these colors as does the normal-eyed, but only that he discriminates with great delicacy slight variations in color. No other subject showed so comprehensive a list of color terms.

O's observations on himself have been prolonged for many years. He has worked out secondary criteria for the determination of colors and, in common with other reported cases, shows great skill in using such criteria. The normal-eyed often find difficulty in getting the meaning of the terms used, such as "sickliness of hue," "texture,"

TABLE 5

NEGATIVE AFTER-IMAGES, SUBJECT O

The color of the stimuli is given along the top of the columns with the usually reported after-image in parentheses. The first column gives the color of the background upon which the image was thrown.

Color of background	Yellow (blue)	Blue (yellow)	Red (green)	Green (red)	Orange (blue-green)	Violet (yellow)
White	blue	deep yellow	whitish-blue	sickly-blue or magenta[5]	sky-blue	luminous yellow
Black	violet-blue	crimson[2]	blue-gray	purple or black-blue[6]	blue	deep crimson[10]
Orange	dark-gray	luminous orange	deep yellow	gray[7]	dark-gray	orange
Yellow	light-gray	orange-yellow	light, sulphur yellow	gray	gray	pure yellow
Blue	blue	light-gray	light bluish-gray, almost white	gray	liquid-blue	gray
Violet	blue	light-gray	light-gray	gray	luminous blue	gray
Red	plum[1]	red, terra-cotta, or yellow[3]	light-gray	light-gray, may be lavender-gray[8]	purple or gray[9]	dark-red or ruby[11]
Green	slate-blue	yellow[4]	dark-gray	light-gray	slate-blue	dull-yellow

Color of after-images reproduced on Milton-Bradley Color Top. The verbal descriptions below are the author's.

[1]4½ yellow + 3 black + 12½ dark blue (purplish gray).

[2]2 yellow + 18 black (dark sage green).

[3]6 yellow + 14 red (dull orange).

[4]Peculiar cast.

[5]Did not persist as long as other after-imgaes.

[6]Blue with red mixed in. A second trial gave *Blue,* perhaps plum. Blue-black tint matched by 3 black + 2 yellow + 15 dark blue, or 6 blue + 14 red (nearly).

[7]Unlike other grays. Impossible to describe.

[8]Suspect tinge of color.

[9]Hard to describe, peculiar cast. Matched by 3 yellow + 2 black + 15 blue shade.

[10]15 black + 5 yellow (olive green).

[11]8 red + 9 black + 3 yellow (brown orange).

and the like. By long study of his case and of the colors which he confuses, O has acquired great skill in distinguishing them. He was at one time quite able by means of such criteria to camouflage his color-blindness in a Holmgren test. In an experiment on negative after-images now to be reported he uses the secondary criteria in a most unexpected way. Since O, when taking the test, had no knowledge of the results anticipated in after-imagery, his suspicion that certain after-images were confusion colors is tremendously interesting and, possibly, significant.

O's conclusions, after a prolonged study of his case, were that he saw, normally, yellow, blues, reds, whites, and their intermixtures. All violets were seen as pure blues, all greens as neutral browns or grays. Occasionally, he suspected that he did not see red as does the normal-eyed. That there is a certain color, rich and individual in quality, unhesitatingly called red whenever seen, was unquestioned. That that "red" might possibly be a very luminous yellow was conceded, but doubtfully. O had, however, read Pole's observations on himself and had been impressed by them. The results of the after-image experiment convinced him that probably his color vision was reduced to blue and yellow. The Ishihara Test gives some evidence of red vision which, however, must be very defective.

The first experiments upon negative after-images tried by O were conducted by himself in 1899, upon explicit directions sent him by the writer.[8] O was at that time ignorant of the probable explanation of after-images and their coloring. He was provided with a Milton-Bradley color-top with the color discs, white, black, blue, violet, yellow, orange, green, and red. He was told to place the stimulating color upon a white background and to throw the after-image upon white. He was instructed to keep his eyes from alien stimulation during the experiment.

I give below parts of a letter received from O in preliminary report.

> "Merely as a matter of interest and as forecasting in a shadowy way what you may look for when I make my final report to you, I shall make a few observations on the partial results thus far obtained.
>
> "Firstly, then, it is going to be extremely difficult even with the color-top to convey to you a correct impression as to the after-images obtained by me from the six primary colors. This

[8] O (W. O. Owen) was at that time a U. S. Examiner of Surveys. His training as an engineer and his natural temperament conjoined to make him extraordinarily accurate in observation and precise in description.

fact will be more salient when I say that I don't even know that I can tell myself correctly.

"For instance, if from a given color I should obtain blue for an after-image, I could give you several different combinations with the top that would answer the requirements. Plain blue, blue-green, or magenta, would each and all convey my impressions.

"As to time, I find that a longer stimulation, even though a shorter one will produce an image, is desirable, as it gives me more lasting impressions and affords more time to study the color of the after-image. I find sixty seconds about right for a good, long, impression. However, 15 seconds will give an image, but it disappears too quickly to be of much use.

"Up to this writing I have made repeated experiments on the six primary colors—red, orange, yellow, green, blue, and violet, simply to get the question properly before me, and the results are of surpassing interest.

"With sixty seconds' stimulation in each case, I find that the after-image for—

violet is yellow—about like the yolk of an egg
orange is bright blue
blue is bright yellow
yellow is sky-blue
green is sky-blue
red is blue—lighter than sky.

"Now, understanding fully that the naming of colors is of little value, I give the above as having some bearing on the report I shall make later on.

"Without comment the above statement as to after-images might give you impressions entirely at variance with the facts.

"From persistent and long study I am satisfied that some of the blue after-images are not blue, but of hues which the color-blind confuse therewith. And this brings us to the perplexing question of what the color-blind actually see. You will observe that orange, yellow, green, and red, all give me blue after-images; and yet, from the peculiar appearance of the image from green I am almost sure it is not blue, but is probably one of the magenta or rose-pinks. Again, the after-image from red is in reality a blue-green, which, of course is blue to me. The yellow after-image from violet may be one of those yellow-greens, and I have a suspicion that it is, for there is a yellow-green which to the color-blind is a beautiful canary yellow. And 'canary' is very nearly the hue of violet's after-image.

. . . . . .

"When I look at a skein of wool which is a certain shade of magenta I say it is blue. You correct me and say 'That is not blue—it is rose color. Your eyes have deceived you.' Knowing you to be normal-eyed, I acquiesce at once and am assured that the color, notwithstanding the blue impression I have received, is magenta (or rose, just as you prefer). Now, are after-images subject to such corrections as this? In the case of after-images, there is, of course, no chance to compare the impression received by the color-blind with that of the

> normal-eyed, and consequently I cannot say positively that any given impression though seemingly blue to me, is magenta, green, pink, etc. But through long years of investigation and observation I am enabled sometimes to detect the colors of confusion from a peculiar cast they possess—a sickly, unhealthful appearance, if I may be allowed the expression; and in my after-images from red, green, and violet, I am almost certain that I discern these indications. It is quite probable that my after-image from green is a magenta pink; from red is a blue-green; and from violet is a greenish yellow."

The interesting part of this report was the subject's suspicion that some of his negative after-images were of the confusion colors; notably that the blue after-image of green was in reality a rose-pink or magenta perceived by him as a "sickly blue." It should be remembered that the subject was unaware of the normal after-images and that his corrections were made from his own prolonged observations of the minute sensational differences of the confusion and other colors.

The experiments that follow were chiefly planned to discover if possible the real color-tone of the after-image from red and green, and to determine, if possible, whether red were seen as by the normal-eyed. In all the experiments conditions were kept constant. O was tested by the writer at the University of Wyoming during May and June, 1900; and again in August, 1902. The results of the two series are grouped together. A comparison of the results obtained at different times gives a simple test of the accuracy of the subject's work. The agreement of records has been astonishing. It was hoped that throwing the negative after-images upon differently colored background would help to determine its color-tone. The results are summarized in Table 5, which confirms O's early report and suggests some interesting questions but unfortunately answers none. The throwing of the after-images upon black, green, and red backgrounds gave, however, interesting results, notably in the case of the red background which apparently dulled the color of the after-image and renders somewhat dubious O's claim of seeing red. The use of the black ground showed that O's crimson is probably a mixture of yellow and black. The experiments should have been carried much further with greater emphasis on light- and dark-adaptation. The latter condition was approximated in the series reported.

Summary and Conclusions

The above report gives data by means of which one may compare

the defects in color vision found in seven members of one family group in three generations. Six of the group are males, one a female.

The Ishihara Tests for Color-Blindness classifies all of the seven as red-green blind, completely green-blind. Other standard tests (Holmgren wool test, Stilling's Pseudo-Isochromatic Plates, Nagel's test) confirm the conclusion of green-blindness on the part of the subjects but suggest inequalities in the degree of red-blindness. Subject T, in particular, shows confusion only for unsaturated red.

There is some indication that the subjects fall into two groups so far as color defect is concerned: O, C, S, R, and Ki (Uncle, O; nephews, C, S; and grandnephews, R, Ki), and K, T (mother and son). K is the niece of O.

The color defects in the first group appear to be very similar and to be transmitted in accordance with Mendelian expectation. O's sister E is apparently a carrier of color-blindness; two of her four sons are color-blind and at least two of her six daughters have transmitted the defect. K, of the second group, may or may not have inherited her color-blindness from the same source as her cousins. Her color defect, although very pronounced, is not precisely comparable to that of her uncle and cousins. The color defect of her son T is less serious than that of the other subjects.

More delicate quantitative tests would no doubt reveal variations in the degree of defect in the seven subjects, differences that it was not possible to identify by the crude methods utilized.

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TROIS GÉNÉRATIONS DU DALTONISME

(Résumé)

Cet article rapporte des données au moyen desquelles on peut comparer les imperfections de la vue des couleurs chez sept membres d'un groupe de même famille distribués en trois générations. Six personnes du groupe sont mâles et une femelle.

Le Test Ishihara du daltonisme appelle les sept daltoniens pour le rouge et le vert, complètement daltoniens pour le vert. D'autres tests bien connus (le test Holmgren de laine, les plaques pseudo-isochromatiques de Stilling, le test de Nagel) confirme le diagnostic du manque d'appréciation du vert mais suggère des inégalités dans le degré du manque d'appréciation du rouge.

Les sujets pourraient tomber en deux groupes: O, C, S, R et Ki (Oncle, O; neveux, C et S; grands-neveux, R et Ki, provenant de la soeur d'O, E), et K, T (mère et fils, provenant de la soeur d'O, H). Les imperfections de la vue des couleurs chez le premier groupe semblent trés semblables et transmises selon l'expectation Mendélienne. E, la soeur d'E, est évidemment une personne qui transmet le daltonisme; deux de ses quatre fils sont daltoniens et au moins deux de ses six filles ont transmis l'imperfection. K, dans le second groupe, aurait peut-être mais non nécessairement hérité son daltonisme de la meme source que ses cousins. Son daltonisme est prononcé, mais non précisément comparable à celui de son oncle ou de ses cousins. Le daltonisme de son fils T est moins sérieux que celui des autres sujets, surtout au cas du rouge.

DOWNEY

DREI GENERATIONEN DER FARBENBLINDHEIT

(Referat)

Diese Abhandlung erstattet Bericht über Daten, mittels derer es möglich ist, die Defekte in der Fähigkeit, Farben zu erkennen (color vision), welche in sieben Mitgliedern einer Familiengruppe, über drei Generationen verteilt, gefunden wurden, unter einander zu vergleichen. Die Gruppe der Farbenblinden bestand aus sechs männlichen Personen und einer weiblichen Person.

In dem Ishara Test für Farbenblindheit zeigen sich alle sieben als rotgrün blind, mit vollkommener Grünblindheit. Andere erprobte Prüfungen, (Prüfung von Holmgren mit Wollen [Holmgren Wood Test], die Pseudoisochromatischen Platten [Pseudo-Isochromatic Plates] von Stilling, Nagel's Test) bestätigen die Diagnose der Grünblindheit, weisen aber auf Ungleichheiten in dem Grad der Rotblindheit hin.

Es mag sein, dass sich die Versuchspersonen in zwei Gruppen teilen lassen einerseits O, C, S, R, und Ki (Onkel O; Neffen C und S; Grossneffen R und Ki, Abkömmlinge von O's Schwester E); und anderseits K und T (Mutter und Sohn, Abkömmlinge von O's Schwester H). Die Farbendefekte scheinen in der ersten Gruppe einander sehr zu gleichen und sich nach Erwartung in Einspruch mit dem Mendelschen Gesetz zu vererben. O's Schwester E scheint eine Ueberträgerin (carrier) der Farbendblindheit zu sein; 2 ihrer 4 Söhne sind farbenblind, und wenigstens 2 ihrer 6 Töchter haben den Defekt übertragen. K, aus der zweiten Gruppe, hat vielleicht, aber auch vielleicht nicht, ihre Farbenblindheit aus der selben Quelle geerbt wie ihre Kousinen. Ihr Farbendefekt ist bedeutend, aber nich ganz mit dem ihres Onkels verglichbar. Der Farbendefekt ihres Sohnes ist minder schwer als der der anderen Versuchspersonen, besonders in Bezug auf Rot.

DOWNEY

THE SPREAD OF NEURO-MUSCULAR ACTIVITY DURING MENTAL WORK*

From the Psychological Laboratory of the Institute of Human Relations†

G. L. Freeman

Much of a theoretical nature has been written about the rôle which muscle tensions play in determining specific drives, attitudes, and overt acts. Actually, there is so little experimentally determined fact that even the cleverest formulations are unconvincing. We may expect, with Head (7), Kempf (8), Lashley (9), Washburn (12), Wheeler (14), and others, that postural patterns are antecedent and fundamental to psychological performances; but until we have a means of investigating such postural accompaniments of mental work, no exact statements can be made about the relationship.

In a previous paper I have suggested that many variations in behavior may be traced to shifts in the pattern of spread of neural excitation. The present report describes a method for studying the activity of five muscle groups in the intact human and exhibits certain patterns of neuro-muscular "spread" which were found to occur under a number of experimental conditions. We have studied, in turn, the general muscular consequences of (*a*) maximal and minimal finger-flexion, (*b*) "sensory" reaction to two intensities of auditory stimulation, (*c*) "sensory" and "motor" reaction sets, (*d*) alterations in verbal and occasional instruction, (*e*) mental arithmetic, and (*f*) practice in learning nonsense syllables and in acquiring a pursuit skill.

With the exception of our study of practice effects, the approach to the above topics has been solely qualitative. We sought to discover what was happening in the quadriceps, arm, and jaw muscles while an individual was anticipating or executing the several tasks.[1] For

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†The research was done while the writer was a National Research Fellow in the Biological Sciences. He wishes to express his indebtedness to Professor Dodge for his helpful suggestions.

[1]Our choice of these particular groups was determined solely by technical considerations. The questions of functional connections between these groups cannot be fully answered. Recent work (10, 13) leads us to expect that what varies the tone of one quadriceps will affect the other likewise.

this purpose it was decided to abandon the method of tendon-deformation, used in previous studies (5), in favor of records of muscle thickening. The subject was placed in a device resembling an ancient pillory or "stock," and displacements of levers, resting on the muscles, were photographically recorded. It may be said in advance that our use of this device was more exploratory than definitive. We were interested in the development of a suitable method for studying the spread of neural excitation and its application to the problems of mental work. It is believed, however, that these preliminary results will command the interest of many psychologists and physiologists who are concerned especially with the description of mental activity in terms of its motor accompaniments.

Apparatus

Figure 1 shows the subject seated in the apparatus, with his legs stabilized at the knee and his wrists fastened to an adjustable board. Three-foot wooden levers, with knife-edge fulcrums, rest over the bellies of the quadriceps muscles and the flexors of the fingers. The pressure of the levers on the muscles was about 30 grams. The movable end of each lever connects with an optical system which transmits its displacements (magnified about 500 times) to moving photographic paper. The details of the Dodge optical system are essentially the same as described in a previous communication (5). A thread from the lever encircles a brass cylinder (upon which a concave mirror is mounted) and is held under tension by a rubber band. In Figure 1, the place where the four cylinders are mounted is indicated by an arrow. The recording camera is not shown in the picture.

Thickening of the jaw muscles could be recorded only indirectly. The most successful of the several devices which we tried is shown in the figure. The subject's upper teeth rest upon a wooden biting board and his lower jaw is held open slightly by a pad of soft rubber fastened to the board. As the jaw muscles contract, the lower teeth press into the rubber pad. This upward movement is registered photographically as follows: A light metal strip rests firmly against the chin so as to move with it. This strip is connected by string to a recording lever placed before the camera. Tension of the jaw-closers is thus reflected by the passage of the shadow of the recording lever across the camera slit. The chief disadvantage of this method lies in its relative insensitivity. In those experiments

where it was used, the recorded changes in jaw tension (white line) bear no quantitative relation to the recorded changes in the limbs (dark lines). The latter are presumptively of equal sensitivity for equal thickening.

Reaction keys and other devices incidental to the execution of the several tasks were appropriately clamped upon the subject's arm rest. Whatever need be said concerning these details will be discussed in connection with the specific experiments.

The Experimental Data

Maximal and Minimal Finger Flexion. Figure 2 shows the spread of neuro-muscular activity in the arm and quadriceps groups during maximal flexion of the right index finger. Figure 3 reproduces a record taken immediately following, in which the subject was instructed to flex the same finger very slightly. Records such as these are typical of many obtained from subjects practiced under both instructions. The reader will note at once that the pattern of neuro-muscular spread varies with the intensity of contraction. Under the set for maximal finger flexion, we find not only heightened tension in the flexors of the right digits, but also corresponding thickening in the left flexor group and in both quadriceps muscles. Under instruction to minimal contraction, little if any thickening is recorded in other than the acting group.

Parallels of this phenomenon probably exist in the more complex reaction systems. A preliminary generalization may be ventured as follows: Above a certain intensity of stimulation, neuro-muscular activity originally confined in some specific pattern spreads to other and more remote reaction systems. We conjecture that the spread will be greatest to systems closely associated with the original action-pattern and least to those whose physiological connection is less permanent and fundamental.[2]

Sensory Reaction to Two Intensities of Auditory Stimulation. Our study of the sets for maximal and minimal finger-flexion suggested that somewhat similar differences in the spread of neural excitation

[2]Knowledge of the functional connections between various muscle groups is too limited to permit its application to this problem. We believe, however, that a thorough study of "associated" reflexes and movements will reveal promising leads. Fairly complete summaries of what is already known about these topics are available (11, 1). May we not expect that future developments in this field will show the organization and differentiation of specific muscular patterns from a smaller number of common matrices.

would be revealed in the field of sensory reaction. Common observation supports the assumption that it is more difficult to react to an auditory stimulus near the threshold than to one well above it. Our problem became the comparison of the muscular accompaniments of the preparatory sets for two widely separated intensities of stimulation. Such stimuli were supplied by the "click" of the diaphragm of a radio ear-phone when an electric circuit was closed. The phone was placed in series with the secondary of an induction coil. Two dry-cells were connected with the primary coil, with a condenser shunted across its poles. The phone was situated so that a reflection from a mirror resting over the diaphragm was focused upon the camera slit. This mirror chip was attached, after the manner developed by Dodge, so as to oscillate with the diaphragm. Photographic registration of both the incidence and intensity of stimulation was thus made possible.

At every ready signal, the subjects were either set for "weak" or "loud" stimuli. Figures 4 and 5 reproduce records which are fairly typical of those obtained with three subjects. The stimulus line is easily recognized in these figures. The narrow line immediately above the time marks (in white) shows (*a*) the moment when the subject was given the ready signal and (*b*) the moment when he released the reaction-key. The intermediate irregular lines show the thickening of the four muscle groups during the period of anticipation.

The records obtained in this experiment indicated that the spread of neuro-muscular activity is much more pronounced when the subject is prepared to react to a stimulus near his auditory threshold. In two of the subjects, the increased spread was largely confined to the quadriceps groups (see the figures). The records of the other subject showed no uniform and stable pattern, increments in thickening being highly variable among the groups studied.

These data are in agreement with the results obtained in our study of maximal and minimal finger-flexion. In both cases we find an increased spread of neuro-muscular excitation in preparation for the more difficult of two performances. Whether or not these "sets" are actually facilitative cannot be demonstrated upon the basis of so limited a study. Work now in progress is concerned with this fundamental and important question. The present report emphasizes the fact that there are recognizable differences in the postural preparation for two degrees of difficulty in performance.

Sensory and Motor Reaction Sets. The previous explorations brought us to a very difficult and intriguing problem, namely, the investigation of the postural accompaniments of introspectively different sets and attitudes. It seemed advisable to begin our study by an analysis of the traditional "motor" and "sensory" reaction sets. The three subjects were practiced (100 trials) in both forms of set before comparative records were taken. We soon found that the "motor" set (attention on the reaction) was largely confined to the acting muscle group, whereas the "sensory" set (attention on the stimulus) involved highly variable spreads. Beyond this important preliminary result, no further generalization can be made. We realized that the thorough investigation of the problem must include records of antagonistic muscle action, for which the present apparatus was not designed. Williams (15) has reported that the pressure on the reaction-key tends to vary with the set adopted. In the motor set, the gradual lifting of the finger from the key during the anticipatory period was frequently found. The sensory set sometimes led to an antagonistic reaction (pressing the key down prior to its release). Records obtained, after the method of Dodge and Bott (3), from the reciprocal muscle groups involved in holding the key may show more specific differences in these two sets.

Figures 6 and 7 indicate fairly typical differences revealed in our exploration of the two reaction sets. In both records the ready signal is shown by the small dot near the bottom. The reaction line and the thickening records are self-evident. The dim white line in the middle was produced by the shadow of the jaw-tension lever. In Figure 6 (motor set) we see the neuro-muscular pattern confined almost solely to the acting muscle group. With the ready signal, there is an irregular increase in thickening, culminating with the sudden raising of the knuckle[3] from the reaction key. Figure 7 shows that the spread is more general for the sensory set.

Alterations in Verbal and Occasional Instruction. Our interest in the postural accompaniments of introspectively different attitudes led us to attempt a number of experimental variations, only the most suggestive of which will be reported here. It is generally recognized that sets and attitudes may be initiated by one of three means: (*a*) verbal or formal instruction, (*b*) self-instruction and (*c*) occasional instruction (arising out of the situation). Of these

[3]This method of holding the key was necessary in order that the volar surface of the arm would remain uppermost as in the other experiments.

the first and last means were within the scope of our experimental method.

A most provocative lead is to watch for changes in postural patterns when the formal instruction shifts the subject from one task to another. Complications in the matter of interpretation are avoided if the tasks chosen are such as are likely to involve the muscle groups under survey, and if the instruction is changed prior to the actual execution of the task first assigned. Two types of choice-reaction proved admirably suited to this kind of study. Under the first set the subject was instructed to flex his right index finger for a white light, his left for a red light. Under the second set he was instructed to flex his toes appropriately instead. After some practice the subjects came to know both sets as Instruction 1 and Instruction 2.

What happens to the neuro-muscular pattern of a person set for Instruction 1 (finger-reaction) when suddenly asked to react under Instruction 2 (toe-reaction) or vice-versa? The answer is given in Figure 10, which reproduces a typical record taken under these conditions. At the first signal, we see the subject's preparation to react to the lights by flexing a finger. At the second signal, he is told to prepare to flex his toes (the actual instructions were: "I've changed my mind, flex your toes instead,") and at the third signal the choice toe-reaction is made.

An attitude which may be easily studied by way of altered experimental condition is that of fright or "startle." The subject was allowed to rest with closed eyes for some time. Suddenly a flashlight powder was set off. The neuro-muscular consequences of such a situation are shown in Figure 11. These are two sections of a continuous record. The first section is characteristic of the subject at rest. The second section was taken at the time of the flash. We obtained, of course, a commentative report which revealed that the subject was actually startled.

The postural accompaniments of "expectancy" were also explored. Figure 8 shows the pattern of neuro-muscular activity during a period when the subject expected a shock to be delivered to his right index finger. The first signal indicates the time when the subject was warned and the second signal shows when the shock was actually given. This record should be compared with that of Figure 9, taken on the same subject at a time when he had no prior expectancy of shock. We see that giving a shock upon such a background of unpreparedness increased the neuro-muscular activity following the in-

FIGURE 1

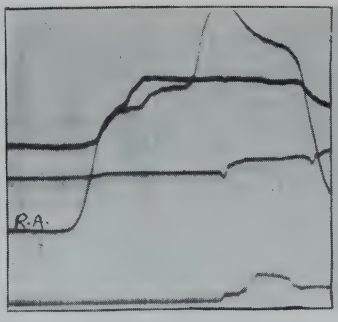


FIGURE 2

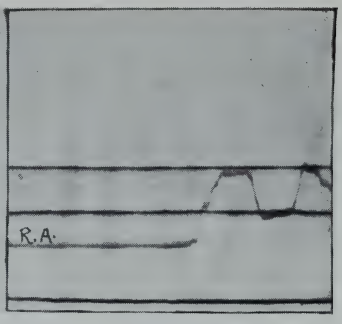
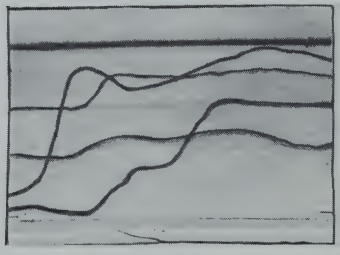


FIGURE 3



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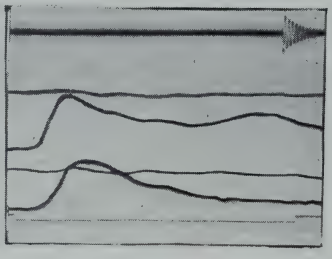

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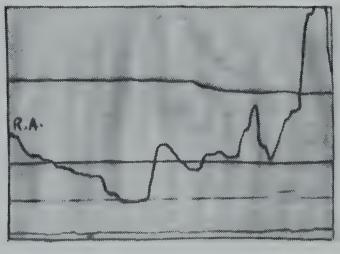
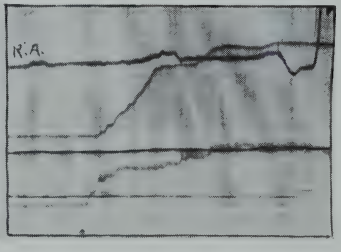


FIGURE 6



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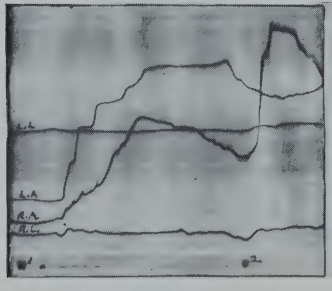


FIGURE 8

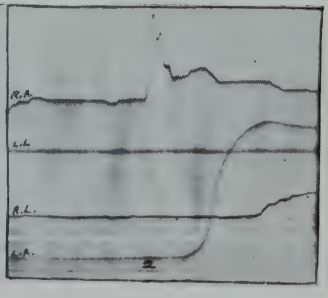


FIGURE 9

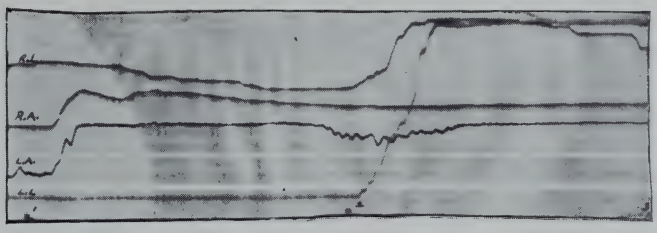


FIGURE 10

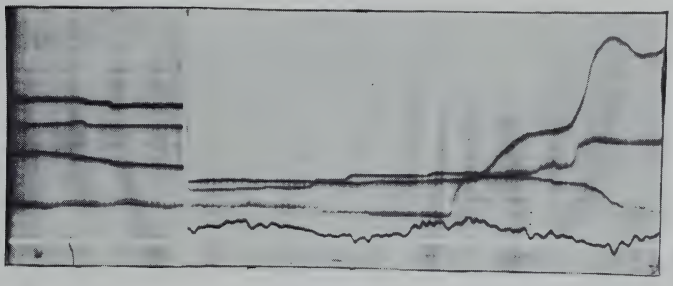


FIGURE 11

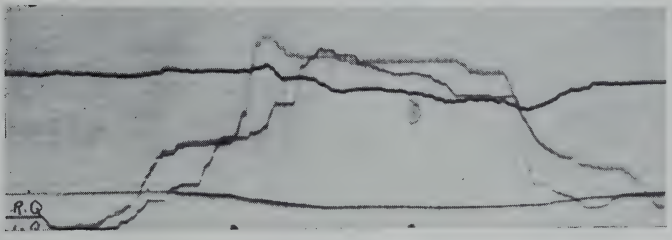


FIGURE 12

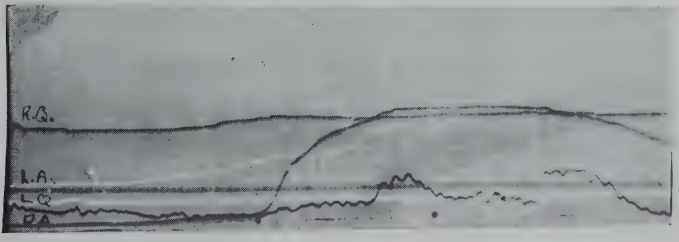
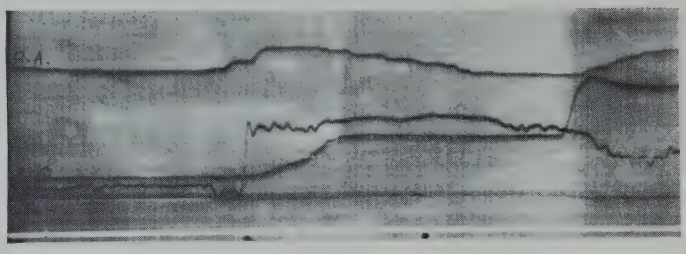


FIGURE 13



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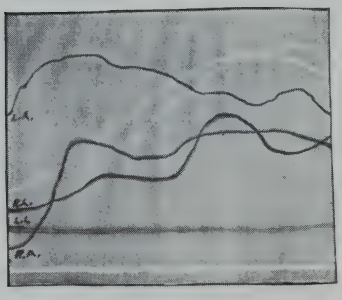

FIGURE 15

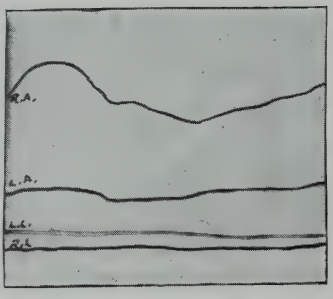

FIGURE 16

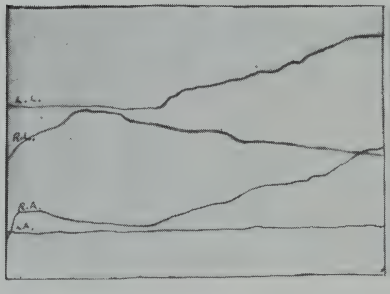

FIGURE 17

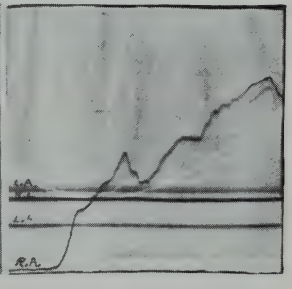

FIGURE 18

sult. Preparation or expectancy of the shock (Figure 8) increased the anticipatory tension but decreased the amount of activity following.

We have only to look about us to see the pertinence of this observation. In meeting almost any type of emergency we find less energy likely to be expended by the prepared than by the unprepared individual. Certain additional information upon the rôle of expectancy in the economy of central nervous operation will be found in one of the writer's earlier articles (4).

Mental Arithmetic. Are there specific postural patterns which accompany various types of cerebration? The possibility is frequently suggested in the motor theories of consciousness. While realizing that existing means of recording are too gross to provide crucial data on such a problem, we made a preliminary study. In a series of casual experiments, subjects were required to utilize the four fundamental arithmetic operations in accordance with dictation such as 5, take away 2, multiply by 3, etc.

Figures 12, 13, and 14 reproduce records taken in close succession upon the same subject. These records are typical in showing that the pattern of muscular response (as recorded by our apparatus) is extremely variable during mental arithmetic. In the first record, the major amount of neuro-muscular activity is apparently localized in the quadriceps groups; but in the other two records, the arm muscles show the greater thickening. As was suggested in the paragraph above, these records cannot be said to negate the possibility that more specific foci are somewhere existent. They do indicate, however, that the supporting (peripheral) pattern of neuro-muscular activity is in almost constant flux.

Practice Effects. What changes occur in the neuro-muscular pattern of performance as the result of practice? The genetic studies of Coghill (2) on salamanders indicate that specific action patterns develop out of diffuse and more generalized reactions. Common observation of the child learning to write also supports the statement. It seemed that our apparatus was suited to the experimental consequences of practice in the adult human. The manual pursuit of a visual object in sine-wave motion provided a skill which could be readily acquired in connection with our experimental setting.

The mechanics of the system by which the tip of a lever was made to oscillate before the camera slip need not trouble us. The subject watched this (stimulus) movement and followed it with an ad-

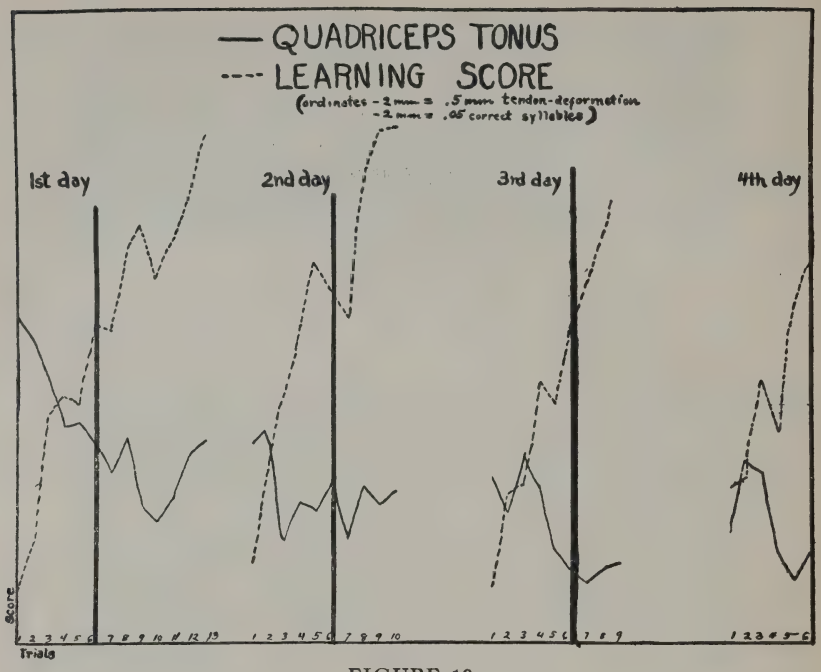

FIGURE 19

jacent (reaction) lever, suitably connected with a stick held in his right hand. Since the manual pursuit could be executed by finger and wrist movements, the right arm could be held stationary for recording as in prior experiments.

Figures 15 and 16 show the muscular accompaniments of a subject at the beginning and end of extended practice in pursuit. Figures 17 and 18 reproduce similar records obtained from a different subject. In these figures, the shadow of the stimulus lever (in white) is easily recognized as the smooth curve. The pursuit line is more irregular. Inspection shows that at the beginning of the training pursuit is poor and the muscular accompaniments more or less general to the four groups under survey. With the acquisition of skill, however, neuro-muscular activity tends to be localized in the muscle group specifically involved in the task.

The reduction of neuro-muscular activity as a consequence of practice seemed so significant that a further experiment was designed to study such changes quantitatively. Seven naïve undergraduates were

TABLE 1

THE AVERAGE TENSION AND LEARNING SCORES FOR SEVEN CONSECUTIVE DAYS

Days	Ave. mm. tension for *S*'s							Learning scores for *S*'s						
	C	W	D	Fo	L	M	Fa	C	W	D	Fo	L	M	Fa
1	11.5	27.0	11.5	16.8	7.9	10.7	7.9	20	13	18	13	24	13	17
2	10.6	15.1	18.6	5.1	7.5	6.7	4.4	18	10	14	10	18	15	15
3	17.0	6.8	4.2	5.2	8.1	4.3	1.8	13	10	13	9	12	13	15
4	21.1	4.7	.8	4.5	7.7	5.8	.9	17	7	10	6	14	12	15
5	12.4	4.5	3.4	3.7	9.5	5.5	1.2	10	3	10	8	10	7	12
6	5.0	5.3	7.6	.8	7.	2.1	.5	12	5	8	6	12	10	8
7	8.2	4.7	1.2	2.6	5.8	4.	2.3	9	3	6	5	13	7	9

each required to learn a list of nine nonsense syllables on seven consecutive days. The lists were presented in counter-balanced practice order. The method of learning involved successive exposure of each syllable, with the subject attempting to anticipate the next in order before it appeared at the opening. As many trials were given at a session as were necessary for complete mastery of the list. Each of the subjects worked at the same hour on each experimental day.

As a matter of convenience and precision, continuous photographic records of tonus were obtained from the right quadriceps muscle only and by the more sensitive method of tendon-deformation instead of by muscle-thickening.

The tonus increments (over the pre-work relaxation baseline) were read (in mm.) at every second mark. These readings were tabulated first according to the average increment (per sec.) for the entire session and then according to the average for every trial. Table 1 gives the average tension for each subject per day, together with the learning score (number of trials necessary for complete mastery of the list). Figure 19 compares the composite learning scores (number correctly reproduced) with the composite tonus increments per trial.

In Table 1 we have quantitative evidence of a reduction in quadriceps tension as a consequence of practice in learning nonsense syllables. The results indicate that not only are fewer trials taken for acquiring the lists, but also that there is less supporting neuromuscular activity.[4] The same conclusion is reached when we examine the data taken trial by trial. Here the problem is to compare the

[4]The theoretical importance of this condition has been discussed in connection with an earlier article of this series (6).

number of syllables correctly anticipated in each trial with the average amount of accompanying quadriceps tonus. Composite curves, such as those given in Figure 19, are obtained by averaging tension and learning scores for as many trials of each day as there were subjects participating. The composite scores per trial are shown for four days only. Further compositing was deemed impractical, because after the fourth day some subjects were mastering the lists with very few repetitions. In the figure the heavy vertical line is placed after the sixth trial of each day's record to aid the reader in making comparisons. We note the tendency for the spread of neural excitation (as measured in the right quadriceps muscle) to be inversely related to the learning score. The unpracticed performance seems to be characterized by greater energy expenditure and minimal returns. Under habituation, the spread of neuro-muscular activity is noticeably reduced and the work-output is increased.

Discussion

Our general experimental method is to be taken as one of the possible modes of attack upon the problems of "set," "attitude," and other changes in performance produced by instruction, habituation, and the like. The results indicate that many of these problems, which have heretofore been approached solely from the theoretical and introspective angles, are capable of psycho-physiological attack. While they cannot be said to verify any particular hypothesis, they do provide evidence of the general fertility of "motor" theory.

Methods are still far from being adequate to the study of the organization and integration of the muscles into systems and sets; but the data included in this report show the significance of our mode of attack. Photographic registration of the thickening of several muscle groups provides valid and important evidence of the spread of neuro-muscular activity during mental work. We have exhibited records showing variations in the muscular patterns accompanying different sets and attitudes. We have found that the amount of "spread" is a function of the set, and we have been able to show that, under habituation, general neuro-muscular activity is reduced as the pattern becomes more specifically localized.

It seems, therefore, that we may safely predict that the further study of patterns of muscular activity will place on firmer ground those aspects of motor theory frequently discussed as *associative movements* (Washburn), *postural schemata* (Head), and *postural substrates* (Lashley).

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L'ÉTENDUE DE L'ACTIVITÉ NEURO-MUSCULAIRE PENDENT LE TRAVAIL MENTAL

(Résumé)

Ce rapport décrit une méthode d'étudier les phénomènes musculaires qui accompagnent le travail mental. On a placé les sujets dans un appareil pareil à l'ancien pilori ou "brodequins" (Fig. 1) et on a fait des photographies des changements de l'épaississement de divers groupes musculaires. On reproduit ici des résultats types faits dans une variété de conditions expérimentales. De ces résultats préliminaries, on tire les conclusions provisoires suivantes: (1) que l'étendue de l'activité neuro-musculaire est une fonction de la position préparatoire, (2) que les positions de réaction "sensorielle" incluent des ormes plus diffuses d'étendue que les

positions "motrices," (3) que l'attitude de l'expectation augmente l'activité neuro-musculaire avant la stimulation, qui tend à décroître après, (4) que l'étendue de l'activité pendant le travail mental est très variable même chez le même sujet et (5) que l'exercise dans l'apprentissage des syllables non-sens et la poursuite manuelle cause un diminution de telle activité néurale.

FREEMAN

DIE VERBREITUNG DER NERVEN-MUSKELTÄTIGKEIT WÄHREND DER GEISTIGEN ARBEIT

(Referat)

Diese Abhandlung beschreibt eine Methode zur Untersuchung der Formen der Muskeltätigkeit welche geistige Tätigkeit begleiten. Die Versuchspersonen wurden in ein Apparat hineingelassen welches einem alten Pranger oder Block (pillory or stock) glich (Fig. 1), und Veränderungen in der Verdickung (thickening) verschiedener Muskelgruppen wurden photographisch registriert. Es werden Musterregistrierungen (sample records), welche unter verschiedenen experimentellen Umständen gemacht wurden, reproduziert. Aus diesen vorläufigen Resultaten werden folgende provisorische Schlüsse gezogen: 1) dass die Verbreitung der Nerven-Muskel-Tätigkeit eine Funktion der vorbereitenden Einstellung (preparatory set) ist, 2) dass Einstellungen welche sensorische Reaktionen erwarten (sensory reaction sets) diffusere Formen der Verbreitung erweisen als es 'motorische' Einstellungen tun, 3) dass die Einstellung der Erwartung die Nerven-Muskeltätigkeit vor der Reizung vermehrt, und dass diese Tätigkeit danach eine Neigung zur Verminderung zeigt, 4) dass die Verbreitung der Tätigkeit während der geistigen Arbeit höchst variabel ist, sogar bei der selben Versuchsperson, und 5) dass die Uebung im Erlernen sinnloser Silben und in der Verfolgung eines Gegenstandes mit der Hand (manual pursuit) eine Verminderung dieser Nerventätigkeit verursacht.

FREEMAN

THREE RECENT ATTACKS ON ASSOCIATIONISM*

From the University of Toronto

W. LINE

Psychologists have become so used to the charge that their field is characterized by controversy, rather than by harmony, even in regard to its basic concepts and subject matter, that they themselves are often guilty of accepting the situation thus implied as inevitable. Indeed, they appear to glory in furthering the cause of individual systems, each interested in disposing of other—and, therefore, less insightful—points of view, rather than in searching for the fundamental similarities and truths in apparently diverse doctrines. The present article may be said to constitute a plea for more adequate and widespread consideration of the basic harmony manifested in several important emphases at the present time.[1] Special attention will be paid to (*a*) the interrelation of the work of Kelley and that of the London school in the field of individual differences; (*b*) the recent contributions of Lashley and their bearing of the configurationist and noegenetic doctrines. The trend of these significant aspects of modern

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[1]Many similar pleas—explicity stated or implied—constitute a very suggestive characteristic of recent psychological literature, despite the predominance of controversy. From this point of view, the perpetuation of the series *Psychologies of 1925, of 1930* (20, 21), etc., affords ample scope for interesting speculation—whether, for instance, we shall soon be ready for *The Psychology of* . Some of the contributors to the current number of that series appear to have concluded that the present volume should be so named —either for the traditional reason that their "School" is the dominant one, or because their system is the most eclectic, and, therefore, the most comprehensive. So, too, the rapid increase in the amount of historical literature that has taken place in recent years shows the growing consciousness of inter-penetration among the various "theories"; and, in spite of the added controversy on this point, illustrated by Boring's contention of eclecticism versus the configurationist insistence on revolutionary "insight," there is, none the less, a greater realization of basic harmony rather than of insurmountable discord. Of the many articles pertinent to this tendency, the following few will suffice for present purposes: Calkins (3), Bentley (1), Spearman (27, 29). See also Bott (2) for a systematic exposition of such tendencies in terms of the fundamental characteristics of scientific thought.

psychology will then be examined in the broader setting of the whole field of mental science.

I

The most recent work of Kelley (11) is undoubtedly a very significant pronouncement. Its purpose is ambitious; and the evidence there brought forward is of a striking and far-reaching nature. But, without in any way detracting from the originality that Kelley shows, it is important to note carefully the relationship between this and other allied productions, and to view it in its just perspective by recalling the historical development of the testing movement. The urgent necessity of so doing is in itself one of the highest tributes that could be paid to the author, since his book stands out so markedly as to need careful re-integration on the part of the reader if its true significance is to be realized.

Accordingly, we may note that Galton's stimulus to the study of individual differences had two major effects, traceable through two fairly distinct lines of later experimentation. The one led to the empirical attack upon the problem of securing tests that would differentiate the innately superior from the inferior, in respect of basic intellectual abilities; and American and European psychologists—among whom Binet is justifiably singled out as the most notable—have been busily engaged in this task ever since. The second, contributing also to the empirical movement in some degree, even from the beginning, is none-the-less distinguishable because of its greater emphasis on, and specialization of, mathematical correlation. While the whole field of mental testing has necessarily made use of correlations to some extent, especially since their value in directing test construction was demonstrated, the more specialized development has sprung from the fact that Galton's early suggestion (7, 8) contained the vision of mathematical objectivity applied to the problems of general psychology.

This second stream has, of course, not only contributed to, but received stimulus from, the first; none-the-less, it has rigidly adhered to the view that only by the perfection of reliable mathematical criteria can the inner constitution of mind and mental capacities be revealed. At least, what speculative interpretations, what psychological concepts, what functional unities are formulated, must always harmonize with the evidence of objective measurement. We must theorize on our data; but theory must not go too far beyond

those data without still further objective check. Not the measurement of "intelligence," of "special capacities," of "temperament," etc., but to demonstrate the existence, to disclose the inner meaning of "intelligence," of whatever abilities can be isolated, are the goals. Measurements—accurate and interpretable—must follow, for they can never precede.

We may, undoubtedly, place the work of Thorndike, inspired by that of Cattell, and, in turn, by that of Galton, as exerting a very potent influence on this development. But one of the most significant events in recent psychology was the publication, in 1904, of two articles by Spearman (22, 23), one of which demonstrated the need of a mathematical methodology, and supplied certain fundamental tools; the other applied those same devices to an experimental investigation that was much more extensive and more carefully directed than most previous allied researchers, although it naturally lacked many of the finer points of present-day experimentation. How those methodological innovations have developed, have been refined, and have been applied to an ever increasing body of data, can be traced in the periodical literature. But a summary and synthesis of this progress was published by Spearman in 1927 (26).

Previous to this publication, the same author had set forth what he conceived to be the fundamental principles governing cognition (24). To those who had not followed the progress of experimental testing, especially of the mathematically-directed type, this book might very easily appear open to grave criticism; for, despite the very clear and careful statement of a fundamental view of cognition therein presented, we have recently become unfamiliar with such dissertations divorced from concrete facts and large bodies of figures, collected under the controlled conditions of the laboratory—a tendency that ought to be recognized explicitly because of its grave limitations. Not that experimental evidence is entirely absent from the work in question. On the contrary, nothing is propounded without adequate experimental support. But after Spearman's second book had supplied the more objective counterpart, the attention of psychologists was attracted so strongly towards the possibilities of this novel methodological approach to facts of mental constitution that the first volume was temporarily forgotten, because misunderstood in some circles; and a new stimulus was given to the re-examination of old data in the light of the tetrad criterion, and to the checking of the findings in new fields of mental testing. So far, the resultant

amount of controversy has been surprisingly small, and has continually decreased; and, in consequence, many psychologists are returning to the first book, since they now realize that it was logically later in time than the second, and was an interpretative supplement to an objective pronouncement. In itself it might conceivably be open to criticism in some quarters; but if it originated in the experimental field, and if it has in turn contributed a directing influence in that field, its truly scientific value is at once obvious. In any case, it forms an integral part of the only existing system of psychology which admits of adequate integration of the field of individual differences with that of general psychology.

The fundamental fact concerning Spearman's work is that however our theoretical approaches may vary, his objective contribution is independent of such variation; and if we trace the development of his method in the periodical literature of the past twenty-five years, we are at once struck by the remarkable objective purity of the successive articles. Throughout, the experimental findings are the beginning and the end of the contributions. And to offset any possible misinterpretation in terms of equivocal concepts, an algebraic symbolism—*g, p, o, s, w,* etc.—has been employed. It is true that a sub-theory has been offered—in terms of mental energy, inertia, engines, etc.—as a verbal picture that is of value as a tentative hypothesis. But this aspect is of very secondary importance as a statement of conviction. The main theory is objective; the sub-theory has value only so long as it harmonizes with the facts and assists in the systematization of those facts for purposes of clarifying and directing research, and of interpreting the behavior of the organism from all possible angles.

Now we come to Kelley's recent book. An outstanding characteristic of the manner in which the author presents his very significant findings is the critical attitude, adopted throughout, towards the work of the Spearman school. This is especially the case in the first chapter, where, in discussing the point of view underlying his researches, Kelley seems to go out of his way to attack Spearman from every conceivable angle. The reader is thus immediately involved in what appears to be a very controversial matter, and naturally inquires into the points wherein these two scientists disagree.

Yet what does he find? Careful examination reveals absolute agreement on almost all the main points at issue! The initial statement of Kelley's methodological point of view is noteworthy mainly

because of its identity with that given by Spearman in 1904. The need of an adequate technique and its fearless application to a wide sphere were the essential demands then, as now. Even the three criteria stated by Kelley (11, p. 4) were those first emphasized (and made possible by the origination or refinement of the necessary mathematical devices) by Spearman. That the technique should be unconcerned primarily with the way that diverse schools of psychology have conceived their fundamental claims, is essential to such an approach (11, p. 2). And, lest the theoretical discussions contained in the first five chapters of Spearman's *Abilities* should have given the impression to Kelley (as appears possible on page 2 of his book) and others, that interpretation initially directed experimentation in Spearman's laboratory, we would reiterate that a compiled presentation of the results of over twenty years' research must not be misjudged because a logical order seems best for purposes of coherence. The final criterion is always the objective one; and this stands, despite all differences of theoretical opinion.

This identity of outlook is at once illustrated in concrete form if we examine the techniques actually adopted by Kelley. These, unquestionably, are based on Spearman's previous work, together with that of Garnett. They start from the Spearman procedure of tetrad differences—the essence of the method consisting in seeing how much the correlations respectively depart from making the tetrad differences zero. What is significantly new is not (as seems to be implied on page 7) the discovery of a less tedious series of mathematical tools; neither is it the making possible the isolation of facts different in type from those accessible to pre-existing techniques; nor, again, the actual study of a larger number of variables at one time than is the case in Spearman's researches. The novelty is the result of a somewhat unique approach; for, whereas Spearman starts from a number of abilities which have not indicated the presence of group factors, and thence introduces the disturbing elements, one by one, Kelley appears to start from a more complex setting. Undoubtedly, this difference will arouse several interesting problems for future discussion and solution; and equally obvious is the fact that it appears to constitute a most significant advance. But our concern is naturally with the results that have thus far been obtained by these two approaches.

As to these, Kelley himself admits that his findings corroborate

all the basic ones of the London school. Indeed, one of the most important statements in his book is found in the preface. It reads:

> "Incidentally, not a few of the findings of my study, which, it was thought, would be new, are foreshadowed or specifically cited in Spearman's last work. . . . This is really most fortunate, for it lays the foundation for future work with the promise of fruitful outcome not possible without such corroboration."

Or again, quoting from Chapter One:

> "The reader must not conclude because of criticisms that have been made of Spearman's technique *and interpretation* that there is wide disagreement between his writings and those of the present writer. On the whole the two sets of findings are quite remarkably in harmony, the agreements being in the matter of a spatial, a numerical, a memory, and even a general factor, *though this last is differently interpreted,* and also in the conclusion that a large number of specific motor (probably also sensory) factors exist. There is scarcely a disagreement in the matter of music, purpose, cleverness, and sex, though here the data are inadequate. There does seem to be a real disagreement in the importance and extent of a verbal factor, and in that of a mental speed factor" (11, pp. 12-13, italics mine).

Surely a fuller corroboration of the main theory of two factors could hardly have been expected. Even in respect of the last sentence of the above quotation, the disagreement appears to be either a matter of terminology, or else a minor variation of the admittedly tentative picture presented in Spearman's *Abilities.* The only essential variations are indicated by the italicized phrases—and these refer to *interpretation,* not to experimental evidence. A difference of opinion is unavoidable—and healthy—in such a sphere. (Incidentally, also, it points to the importance of distinguishing between the main, objective, theory of factors, and the interpretative sub-theory, and these again from the qualitative doctrine of noegenesis—at the same time appreciating the scientific significance of the inter-relation of these three aspects of Spearman's work.) What is actually one of the most strking facts in the history of experimental psychology is nowhere brought to light more clearly than in Kelley's results, namely, that necessarily tentative as were the findings of Spearman's research in 1904, and despite all the limitations of that research, the spirit of the method and the general conclusions drawn from the results have

remained ever since; and are now substantiated by a variation in methodology which emphasizes still further the scientific value of contributions so harmonious in their results.

What, then, of Kelley's criticisms, which, unfortunately, tend to obscure the more vital harmony, unless read with some care and discernment? They appear, in every case, to be based upon minor points or misinterpretations. The author himself is quite aware of this in many instances—although perhaps not in all: and the reasons for his vigorous presentation of them will be considered in a moment. But when examined in the broader settings of the scientific field, they become so insignificant as scarcely to be noticed, while still constituting focal points for concentrated evaluation, when the essential harmony is once established.

To make plain this contention, we may consider briefly the chief criticisms that are offered. Some of them have been effectively answered by Spearman (30), and of these we need say but little, except insofar as they are germane to our discussion. In some cases the points are unjust, and are based on accidental misrepresentations. Kelley reiterates, for instance, that the proof of a certain formula (that for the standard deviation of the distribution of a sample of tetrad differences, given in *The Abilities* p. xi) has not yet appeared in print.[2] He takes care to point out (p. 212, footnote) that, whereas the size of a sample in Spearman's work is given as 30,000, one of the probable errors is .007, "thus conclusively indicating that the population for this study was much less than 30,000." (The reference given is to a "typical tetrad" from a sample of 30,000 cases—a sample that Spearman had already discussed on page 153, giving the exact number of cases concerned.)[3] Similarly, the remark on page 37 is unfortunate. If Kelley suggests (as he seems to suggest) that the impregnability of Spearman's statement of his factor theory rests upon his ability to "say things"—which sweepingly explain away all difficulties and exceptions—we repeat that no one realizes more than Spearman the futility of verbal equivocality, and of explanation not based on objective demonstration.

[2]The proof referred to has since appeared (31).

[3]The point of the criticism is difficult to see when the nature of the probable-error value is remembered. An equally futile case would be involved if one took Kelley to task for not stating his probable-error values in all cases, even though the size of the sample and the correlation are given!

It would almost appear as if Kelley had failed to grasp the significance of Spearman's approach as similar to his own, when he says (p. 10): "Professor Spearman argues that "Perseveration as measured by G. E. Müller, is the same as introversion as proposed by Jung." The only place where such a misinterpretation could possibly arise would be in Chapter IV of *The Abilities*. But here the author is expressly advocating just what Kelley now proposes. In any case, it was Jung himself who identified these two concepts (10, Chap. XII). Similarly, when Kelley suggests (p. 5) that because Garnett's term "cleverness" has variable meaning with different interpreters, therefore, our final belief in "cleverness" as a factor must be most uncertain, he raises a point already implied in the device of either resorting to a constant use of inverted commas, where such variable terms are concerned, or, better, to a use of symbols such as "*c*," "*g*," etc. The existence of *c* is surely a matter different from whether *c* is cleverness or not. Incidentally, that whole discussion (pp. 4-5) seems to involve the failure to recognize that subjectivity and objectivity of measures are more definitely indicated by reliability coefficients than by personal antipathy to "judgments." What tests are so "objective" that "judgment" is excluded in practice, especially in the interpretation of the results? Reliability coefficients within the test results, the tetrad or other equally valid criterion determining the identity that can be stipulated in interpretation—these are nowhere more apparent than in the works of Webb and of Garnett.

These are minor points, however. Of more interest and significance is the following. We have already stressed the fact that Kelley's findings corroborate Spearman's main theory *in detail*. General, specific, and group factors—emphasis on the latter being hitherto practically confined to the London school—are not only demonstrated, but isolated and enumerated in almost perfect accord with those of Spearman. Where *g* is attributed to "heterogeneity" rather than to "energy," we are not surprised to find a difference of opinion, since this aspect is essentially speculative. We add, however, that differences of opinion can be judged only in the light of the whole experimental picture. Is the one interpretation more in accordance with all the facts than the other, and does it afford a more directive influence to further work? Does "heterogeneity" mean very much except that, without it, correlation disappears? "Energy" is one possible form of heterogeneity. That's all.

The other points of apparent difference are even more easily dismissed. The supposed smallness of Spearman's samples[4] can hardly be the basis of a sound criticism by Kelley, in the light of his own data.[5] To reproach Spearman on the ground of "large and unknown probable errors" is hardly just, in the light of his unique contribution dealing with that very aspect of psychological statistics (11, p. 15);[6] and to imply an unsound (and unscientific) use of probable errors is even worse (11, pp. 214-215).[7] Finally, to attribute to Spearman a gross neglect of considering age, sex, race, and allied factors as influencing correlation (11, pp. 210, 212, etc.) borders on the ludicrous, not only in the light of Kelley's failure to follow his own advice in most of the results he gives, but especially in view of the fact that Spearman was the first to advocate the need for such elimination of disturbing influences, and has based his work on that very principle ever since (22, 15).

Brief as the above treatment of Kelley's criticisms undoubtedly is, enough has been said to establish their relative unimportance. When this has been made clear, Kelley appears as a critic of a very different order from most other writers who delight in attacking the factor school. These latter fall into two classes. First, there are those who grossly misinterpret the doctrine—apparently through inadequate understanding of it.[8] But most assuredly Kelley cannot

[4]Kelley (11, pp. 15, 191, 274). Kelley's remark on page 274 (where he says that Spearman's point of view "cannot be statistically established by a study of small populations") is ambiguous. But if he refers (as seems to be the case) to the impossibility of demonstrating the *non-existence* of group factors, some misrepresentation is involved. The relevant Spearmanism would be to say that the *existence* of group factors may be demonstrated, if and where they occur. But the *non-existence* is another matter; here, the size of the sample—important at all times—has a much more direct bearing. If errors of experimentation preclude the demonstration of the existence of a group factor with a small sample, this does not preclude the possibility of detecting and isolating such a phenomenon when an increased sample is available. See Spearman's reply (30).

[5]Compare, for example, the enormous number of cases involved in Spearman's researches (certainly over 10,000) with the size of the sample on which Kelley's results are based (300-400). Kelley quotes results from Spearman's laboratory based on samples larger than his own total number.

[6]Spearman's publications (as is well known) are crowded with the investigation, refinement, and application of probable-error techniques, whose significance he was the first to realize. Note further, in connection with this point, Kelley's results on pages 84, 119, and 127.

[7]See Spearman (30) in this connection also. It may be that one of the sources of any apparent discrepancy between Spearman's detailed findings and those of Kelley lies here.

[8]See, for example, Carter's (4) work (especially the earlier part).

be placed in such a category. The second type of critic is one who adopts a distinctly emotional attitude, but brings forward little or no argument that might conceivably contribute towards scientific progress.[9] And, again, Kelley is not at all guilty of this undesirable tendency. If, however, a weakness of his masterpiece might be suggested without presumption, the true nature of his criticism can be more readily seen. This weakness consists merely in the failure to emphasize adequately the significant harmony of his work with that of Spearman. A very natural and healthy rivalry is manifest—but partly obscured by its over-keenness. Recognition of this, however, at once brings to clearer focus the possibility of making what differences do exist—in approach and in results—the basis for cooperative advance, since they provide the invaluable essence of good-natured competition in a field where fundamental harmony exists. The relation of this harmony to other aspects of psychology will be considered later.

II

Dr. Lashley's contributions—especially his more recent publication (17) (see also 16, 18)—have performed two very significant tasks. They have cleared the decks for action, in that they have demonstrated the futility of much of the pseudo-neurology, the atomism of synapses, neurons, bonds and the like, so prevalent in many systems of psychology. Secondly, they have pointed the way to a method of correlating the results of studies in the field of learning with those in the complex realm of individual differences.

The first of these achievements is perhaps the more significant. Although it is negative in one sense, it removes an obstacle from the path of psychologists against which many unsuccessful attacks have been made, and, in so doing, it has ramifications throughout all branches of psychology. Calkins (3) has effectively drawn attention to the "fundamental similarity between certain schools of contemporary psychology which at many other points diverge sharply from each other," the similarity consisting in the common protest against psychological atomism; and cites the original spirit (but not the actual working out) of behaviorism, the *Gestalt* movement, the personalists, and the psychology of the unconscious as the outstanding examples. But, however effective these protagonists of organismic or psychic unity have been, they were—with one exception—always

[9]Dearborn's *Intelligence Tests* (6) appears pertinent as an example.

hampered by the same pseudo-neurology which had become sufficiently entrenched to be accepted as unquestionable fact. Lashley has shown very effectively some of the major weaknesses of that neural picture to which we have become accustomed in psychological textbooks and discourses on learning phenomena.

But what is the alternative neural hypothesis, if the atomistic, specific, conduction-unit, postulates are inadequate? Lashley's reply is in the direction of vagueness—which, he adds, is preferable to erroneous conceptions. The positive side of his treatment of neural mechanisms underlying behavior did not originate with him. Neither does it spring directly from his experimental results. Rather it is the logical antithesis of the firmly entrenched telephone-system theory; and, to the extent that his factual data are directly contrary to the latter, they may be said to support an antithetical interpretation.

How, then, does he arrive at the view he gives us of underlying neural functioning? It appears justifiable to assume that his phraseology, his thought, would be influenced, even if indirectly, by any current developments. Consequently, his hypothesis offered as an alternative to the commonly accepted one, shows distinctly the influence of Child (5), and resembles the physiological counterpart of Köhler's physics.[10]

The Gestaltist picture of neural functioning is the one exception to atomism referred to above. We have said that whenever psychologists found it necessary to speak in terms of the nervous system, they became enmeshed in neural specifics, however forcibly they wished to avoid piecemeal representation of behavior. This accounts for the degeneracy of behaviorism, which, while itself born of the organismic-unity spirit, fell into the trap so carefully laid by physiological psychologists to such an extent that it grossly misinterpreted Pavlov's work, and then proceeded to make the resultant "conditioned-reflex" hypothesis the corner-stone of its structure. As such it became nineteenth century associationism in a more mechanistic and materialistic garb. But the real exception to this tendency is all the more outstanding because of its explicit and emphatic avoidance of a similar calamity. Atomism in any form—as the "bundle hy-

[10]Although Lashley makes frequent mention of Köhler, it is erroneous to assume too close a connection between the works of these two authors save from the point of view of scientific relationship. Actually, of course, the "structural" aspect ("material basis") of Köhler's contributions consists almost entirely of pure physics, with little or no physiology in the sense of regard to facts observed about the nervous system.

pothesis," against which Wertheimer exclaims so forcibly, or as the bond theory of Thorndike, which Koffka so heartily and effectively decries—is anathema to the configurationists; and so strong is their advocacy of unity that they are ready to oppose neurological speculation supporting psychologies of "elements," with a physical view more in harmony with their conception of "undivided, articulated wholes."

But to emphasize the Gestaltists' contribution to neural physics would be to miss the point of the real advance made by them. Their major rôle is (or was) that of psychologists, and would appear to be concerned with the more physical side of their science only for purposes of amplifying and clarifying psychological descriptions and explanations, and for making clear their desire to achieve philosophic unity under the rubric "Gestalt." If we add to this a natural tendency to conform to the modern convention—that psychological explanations are valid only to the extent that they can be paralleled by physicial counterparts—then, despite the fact that the Gestaltists at heart ridicule this homage to materialism, the reason for Köhler's analogies of "potential," "force systems," "neural net-works," and so forth, is clear. Likewise, the distribution of emphasis is equally obvious.

The conclusion is justified, therefore, that Lashley's findings and Gestaltist contentions are harmonious. But the bases of that conclusion are other than those frequently emphasized. Lashley's results make untenable the neural picture analogous to atomism. The Gestaltist points out the inherent weaknesses of atomism from the psychological standpoint, equally effectively. Both Lashley and the Gestaltist turn to an alternative view. The one, as neurologist, is largely confined to the physical field. The other is primarily concerned with the mental aspect of behavior and sets up a positive approach to a psychology of "wholeness." That is his main contribution; the physical analogy is secondary, even though it does homage to a popular tendency. The fact that Lashley's neural hypothesis fits that of the Gestaltist, is undoubtedly true, because inevitable. Both are reactions against the same thing; and, as logical antitheses to the same theory, they are one. And if we wish to characterize their psychological harmony most accurately, we find that it consists in the fact that both are attacking associationism. In this they are not alone.

We come now to the second achievement displayed by Lashley's

contribution, namely, his pioneer work in correlating the two major experimental fields of modern psychology. As he points out, the field of learning has been too long dominated by assumptions concerning bonds, synapses, and the like. Theories of intelligence have frequently diverged from this setting explicitly, and have always done so at some point or other, even though they may have started out with an analytical foundation. The need for scientific harmony here is obvious; and Lashley proceeds to attack learning problems by means of the techniques that have proved so valuable in the field of individual differences.

What is the result? The outstanding indication is, as he himself points out, a substantial corroboration of the "energy" postulate of Spearman. And the more carefully one attempts to evaluate his experimental results, the more one is forced to the conclusion that the two-factor theory is the sole existing theory adequate to synthesize these findings. The correlations quoted on page 61 of *Brain Mechanisms,* compared with the verbal conclusions given on the preceding page, are readily interpretable in terms of *g* and *s*, including the well-known ramifications of relative importance of *g* and *s* in the variable tasks, and of amount of *g* required. But, without some such interpretation, the average correlation in the case of brightness discrimination (quoted in Table VII, p. 61) is meaningless or definitely conflicting in relation to the third conclusion on page 60.

Lashley has not gone far with the technique; and therefore too detailed a comparison between his findings and those of workers in the field of differential human abilities is not yet justified. But the present stage of achievement is undoubtedly characterized by harmony rather than conflict; or these recent neurological advances are most certainly following and corroborating the findings of the London school, in this all-important synthesis of the concepts "learning" and "intelligence." Furthermore, even should the criticisms of Lashley's interpretation advanced by Hunter (9) be substantiated by later researches into cerebral functioning, there is still the "integrative function" (cortically or psychologically conceived) to be accounted for; and this is most effectively approached from the angle of correlation techniques, which, so far, have at least indicated the validity of the general and specific factor hypothesis.

III

We have shown that Kelley and Spearman, adopting very similar—if not identical—points of view, have arrived at a common con-

ception of mental substructure by means of two distinctly different statistical approaches. Kelley has not yet furnished us with a qualitative analysis corresponding to his empirical demonstration, or with a synthesizing sub-theory of mind. But, since Spearman has performed this all-important task, Kelley's results can be unified in a similar way. Indeed, they lend support to that interpretation. The theory of *g* is likewise supported by Lashley's results, insofar as they contribute in that direction. We have seen also that Lashley's neurological contribution harmonizes with the configurationist hypothesis, where these two approaches may be justifiably compared. It remains to point out the more qualitative significance of Gestaltism in relation to other current psychological views—especially in relation to the doctrine of noegenesis.

First of all however, it is interesting to note that the recent developments in the configurationist camp stress more and more the convergence of present-day theories. When Wertheimer attacked the "bundle hypothesis"—with sensism-associationism specifically in mind, and when Koffka likewise criticized the traditional doctrine that "all consciousness consists of a finite number of real, separable, though not necessarily separate, elements, each element corresponding to a definite stimulus or memory residuum" (12, p. 533),[11] particular care was taken to distinguish their point of view from the doctrine of *Gestaltqualität.* Indeed, the tenets of the Graz school, made famous by such advocates as Meinong, Mach, Ehrenfels, Benussi, etc., were explicitly attacked—especially in relation to the problem of the genesis of wholes and whole qualities.[12] Contrary to the Graz school, the Berlin scientists denied the entire conception of "acts of production" and stressed the immediacy of the whole. At the same time, the "experiencing individual," though recognized to some extent, was at first kept very much in the background. The reason for this is obviously bound up with the matter of emphasis. Koffka's main concern, for example, was with an explicit attack on certain phases of atomism. But, when this leads him into the field of learning, "achievement" (13), he is forced to bring in the

[11]This appears to be the first extensive work from the Berlin school, despite their claim to have started in 1912 with the work of Wertheimer. The latter is mainly characterized by the "phi" phenomenon and some rather mild generalization.

[12]This controversy is most clearly set forth and evaluated by Spearman (25). The priority of the Austrian school in the present anti-associationist movement is here given its proper recognition.

personalistic conception of "intention," although its personalistic implications are obscured by other more pertinent emphases. In Köhler (14, especially chapter on "Reproduction"), however, this personalism has become much more explicit, especially through the influence of Lewin's work, where the conception of the self, and the importance of the inner factors in learning, are given chief place, as distinct from the external aspects so fundamental to associationism. It is no longer easy, therefore, to appreciate so marked a division between the Graz and Berlin configurationists as the latter at one time wished to maintain. Neither is it easy to avoid pointing to the basic harmony between the personalists such as Stern, McDougall, Ward, etc. (despite the many differences), since the major difference between personalistic psychology and Gestaltism lay in the differing emphasis on the experiencer. The later Gestaltist writings are making the "personal" concept much more basic than did those of a few years ago.

This being so, the harmony between configurationism and noegenesis likewise becomes more apparent. The latter is obviously very much akin to the binarian view of the Graz school; and basic similarity in the field of sensory perception suggests underlying unity throughout the whole fabric of psychological theory (see 19). If, therefore, the Berlin school is bringing forward an emphasis which implies the "intention," "the activity" of the experiencing self, the gulf between the two configurationist camps becomes less acute—provided that the nature of that "activity" is unravelled. This is unexplained by the Graz school, and passed over—at first denied—by the Berlinists. Spearman's "Eduction" supplies one aspect of the explanation: and his view of conation, which is essentially that so carefully elaborated by Aveling, supplies the other.

Two major difficulties, however, confront explicit recognition of what the writer feels to be a fundamental and complete harmony between Spearman's doctrine of noegenesis and the "Gestalt" hypothesis. The first is one which is overcome merely by recognizing its existence. It consists in the fact that the two approaches differ so markedly—Spearman having worked up to his basic analysis of cognition through the avenue of measurement, while the Gestaltist has concentrated upon qualitative manifestations only. Surely the two, working hand in hand, are essential to scientific progress; and the noegenetic theory is expressly stated to meet the demands of quantitative *and* qualitative considerations. The second difficulty is

in the matter of interpreting words. For example, Spearman's use of the term "relation" is broader than that adopted by Köhler and his supporters; it corresponds with the signification advocated by the Austrian Gestaltists. Otherwise, there is no outstanding point of difference between the two schools of thought, and the distinctions are confined to matters of emphasis. Hence, as Spearman (27, p. 403) himself points out, generous rivalry consequent upon the adoption of differing emphasis, but firmly rooted in a recognized fundamental harmony, would engender a healthy spirit conducive to scientific progress.[13]

Summary and Conclusion

In the first section of this paper, we pointed out that corroboration of Spearman's objective results in the field of individual differences has been furnished by Kelley. Thence we proceeded to Lashley's work, which discredits the whole current physiology of the brain, and at the same time harmonizes with the objective and interpretative aspects of the factor theory. Köhler's claim to have upset the "traditional psychology" of thousands of years can readily be brought into line with these other movements, since on close inspection of what he and Lashley are both attacking, this proves in the main to be Associationism. But such an attack has been the main motif of Spearman's work also. Not merely in the qualitative aspects of his doctrine, but also and especially in his approach to the study of individual differences, and, through this, to the resultant analysis of organismic behavior, does he lay bare the inadequacies of all forms of associationist atomism. He not merely decries atomism; he takes us further—into the constructive field—and portrays mental life in a way that makes practical application of his results immediately possible. His theory has all the advantages of scientific analysis, with none of the inherent errors; for, while preserving the unit of

[13]The special significance of this contention, applied to the field of visual perception, received some treatment by the present writer in the article cited before (19). A more extended application will appear shortly. In the same connection, see Spearman's (28) contribution to *A History of Psychology in Autobiography*, where he reiterates his anti-associationism, rejects *in toto* the current physiology of the brain, and stresses again his advocacy of relations. These latter, he points out, when systematic, are identical with the "forms" that constitute one of the meanings of "Gestalt." This advocacy, however, includes the "as" relations, and, therefore, does not prevent him—as is the case with the Berlin Gestaltists—from accepting also the "that" relations of the Austrian school.

the organism, he accepts the validity of a descriptive picture from several points of view, and hence arrives at his factors. But the factorial analysis—so totally different from the piecemeal separate-element conceptions of associationist views—has no meaning save in terms of "mutual involvement," interrelation, or total-functioning. The explicit recognition of the essential similarity underlying the thought of these leaders in the scientific field would seem to be an important step towards future progress.

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TROIS ATTAQUES RÉCENTES CONTRE L'ASSOCIATIONISME

(Résumé)

Cet article fait valoir la nécessité d'une considération plus adéquate et étendue de l'harmonie fondamentale manifestée dans plusieurs domaines importants de la Psychologie actuelle. Dans ce but, il considère l'oeuvre de Kelley, montrée dans ses "Crossroads," dans ses rapports avec celle de Spearman; et montre que ces deux investigateurs sont arrivés à la même conception de la sous-structure mentale au moyen de deux différentes approches statistiques. Ensuite il discute l'oeuvre de Lashley, laquelle discrédite toute la physiologie actuelle du cerveau, et en même temps s'accorde avec les aspects objectifs et interprétatifs de la Théorie Facteur. L'assertion de Köhler qu'il a renversé la "psychologie traditionnelle" de milliers d'ans, se montre d'accord avec ces autres mouvements; puisque l'inspection soigneuse de ce que celui-ci et Lashley attaquent montre que ceci c'est en grande partie l'Associationisme. Mais une telle attaque a été aussi le vrai but de l'oeuvre de Spearman aussi. Dans les aspects qualitatifs et quantitatifs de sa doctrine, il a montré non seulement les faiblesses de toutes les formes de l'Associationisme, mais aussi il nous a donné une méthode constructrice et une description de la vie mentale d'une grande signifiance pratique. Sa théorie a tous les avantages de l'analyse scientifique avec aucunes de ses erreurs inhérentes; car, tout en préservant l'unité de l'organisme, il accepte la validité d'une peinture descriptive à plusieurs points de

vue, et arrive donc à ses Facteurs. Mais l'analyse par facteurs—si absolument différente des conceptions par morceaux et éléments séparés des vues associationalistes—n'a aucune signification excepté en termes de jeu mutuel, de l'inter-relation, ou de fonctionnement total. La reconnaissance explicite des similarités essentielles dans la pensée de ces savants scientifiques paraîtrait une étape importante vers le progrès futur.

LINE

DREI NEUE ANGRIFFE AUF DEN ASSOZIATIONISMUS

(Referat)

Diese Arbeit befürwortet eine angemessenere und weitverbreitete Berücksichtigung der Wesensharmonie, die in mehreren wichtigen Emphasen der gegenwärtigen Psychologie manifestiert wird. Zu dem Zwecke prüft sie die Arbeit Kelleys, wie sie in "Crossroads" dargestellt ist, in ihren Beziehungen zu der Spearmans; und zeigt, dass diese zwei Forscher durch zwei verschiedene statistische Verfahren auf eine gemeinsame. Auffassungsweise des geistigen Unterbaues kamen. Dann geht sie zur Arbeit Lashleys über, die die ganze gegenwärtige Gehirnphysiologie diskreditiert und gleichzeitig mit den objektiven und interpretiven Gesichtspunkten der Faktorentheorie übereinstimmt. Köhlers Behauptung, dass er die althergebrachte Psychologie von tausend Jahren über den Haufen geworfen habe, wird mit den genannten Bewegungen in Übereinstimmung gebracht. Die genaue Prüfung dessen, was er und Lashley angreifen, ergibt, dass es im Wesentlichen der Assoziationismus ist. Aber auch Spearmans Arbeit hatte dieselbe Attacke als Motiv. In den qualitativen und quantitativen Gesichtspunkten seiner Doktrin hat er nicht nur alle Formen des Assoziationismus als unzulänglich erwiesen, sondern er hat uns zugleich eine konstruktive Methode und eine Darstellung des geistigen Lebens vermacht. Seine Theorie hat alle Vorteile der wissenschaftlichen Analyse ohne deren innewohnenden Fehler. Denn, währenddem er die Einheit des Organismus bewahrt, anerkennt er von mehreren Gesichtspunkten aus, die Gültigkeit eines beschreibenden Bildes und kommt damit zu seinen Faktoren. Aber die Faktorenanalyse,—so ganz verschieden von den Stückweis—Vorstellungen der getrennten Elemente von assozionistichen Betrachtungsweisen,—hat keine Bedeutung, wenn ohne Bezug auf gegenseitige Verwicklungen, Verbindungen untereinander, oder Gesamtfunktionen. Die klare Anerkennung der wesentlichen Ähnlichkeiten, die den Gedanken dieser wissenschaftlichne Führer zu Grunde liegen, ist wohl ein wichtiger Schritt in der Richtung des zukünftigen Fortschrittes.

LINE

SHORT ARTICLES AND NOTES

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DIFFICULTY AS A FACTOR IN THE STANDARDIZATION OF A MAZE[1]

BY L. W. ALLISON

There has of late been considerable attention directed toward the question of the reliability of the maze as a measuring instrument. The efforts along this line have not been without rewards. We now have on record a number of mazes with group reliability coefficients of .90 or above. Little effort has yet been made, however, toward validating the maze with respect to other types of ability. No attempt will be made here to review all the recent literature on the various methods used for determining the reliability of the maze or the results obtained. For a bibliography of the more important studies see Peterson and Allison (5).

In relation to this study it may be well to remind the reader of results of a few related studies. Nyswander (4), in making a comparison of the finger maze and the stylus maze, found that degree of difficulty is an important factor in the reliability of a learning problem. This is, of course, true also of all tests. Hunter (1) found positive relationships (.17±.09 to .45±.08) between performance on a maze and the Otis test. Warden (7) found a correlation of .37±.09 between Alpha scores and maze performance; Spence and Townsend (6), with twenty subjects, obtained correlations ranging from .57±.11 to .66±.08 between scores on the Thurstone Intelligence Test and performance on a high relief finger maze. The reliability of the mazes used by Hunter and Warden was not given, but Spence and Townsend reported reliability coefficients ranging from .90±.03 to .98±.01. Peterson and Allison (5) found correlations ranging from .02 to .23 between equally weighted composite maze-score factors

[1]This study was carried out in the Jesup Psychological Laboratory under the direction of Dr. Joseph Peterson to whom the writer acknowledges valuable assistance.

(trials, errors, time) and Alpha scores. The reliability of that maze ranged from .42±.15 to .96±.01, when periods of visual exposure of 5, 10, and 20 seconds before each trial were allowed. The maze used in the present study differs, at least in pattern, from any of those used in the studies cited above. The purpose was to determine the effect of changing (by visual exposure) the difficulty of a high relief finger maze (*a*) on its reliability,

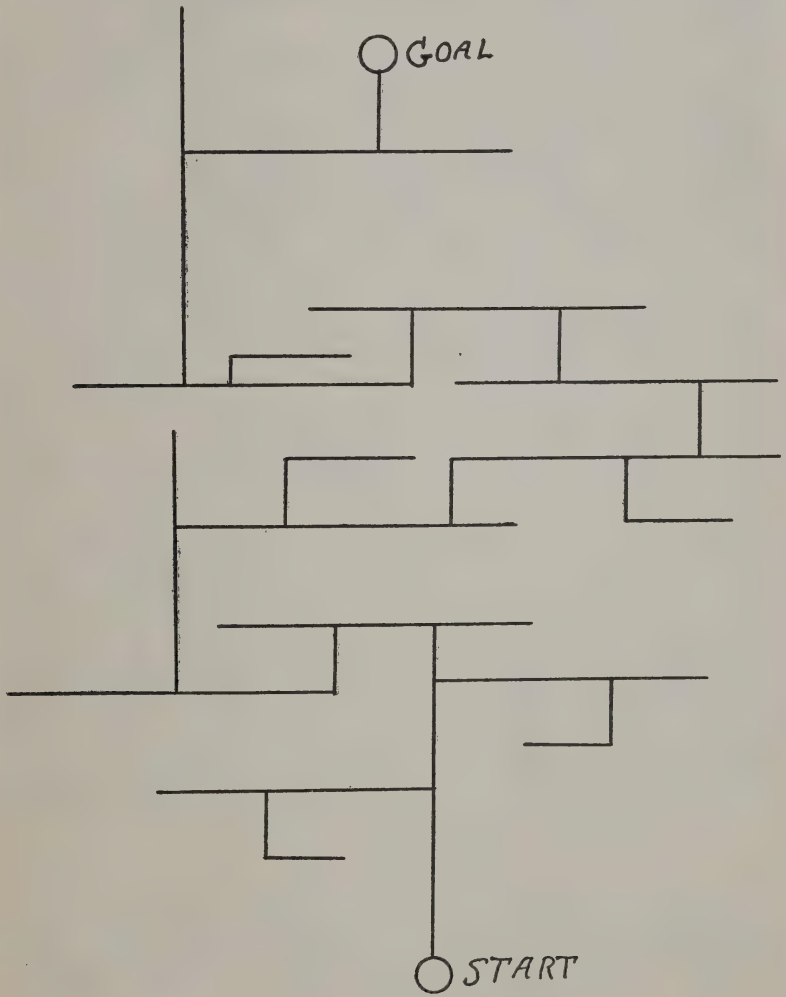

FIGURE 1
PATTERN OF HIGH RELIEF MAZE

and (*b*) on its relation to performance on the Army Alpha test and to efficiency in psychology examinations. The maze was a high relief finger maze made after the manner suggested by Miles (3). No. 14 German-silver wire was used in the construction of the pattern. The wire was bent into square-cornered staples of suitable length for the pattern desired and these were driven into a wooden panel. The panel was 16¼ x 14 in., 149½ sq. in. of which was covered by the pattern. Figure 1 shows a reduced form of this maze.

The instructions (all to be read) and the method were similar to those given in the report of the earlier study by Peterson and the author. The learning was always completed at a single sitting. Thirty-three students (30 women and 3 men) from a class in elementary psychology in George Peabody College for Teachers served as subjects. These subjects were divided into two groups, equated on the basis of their Alpha scores. Group A had a mean Alpha score of 134.55±4.97 and Group B of 135.33±5.97, the difference between the mean of the two groups being only .78±2.26. Group A learned the maze under the usual blindfold conditions, never being allowed to see the maze at any time until the learning was completed. Group B, also blindfolded, learned the same maze by the visual-exposure method. The instructions for Group B were the same as those for Group A with the exception of this additional statement: "Before each trial you will be given 5 seconds to study the maze. During this time, however, you are to make no motion with your hands either over the maze or as if tracing an imaginary maze." The blindfold consisted of a pair of goggles with covered glasses as recommended by Miles (3).

Table 1 gives the reliability coefficients of the maze score. These were obtained by correlating the total number of errors on the odd trials with the total number made on the even trials. Peterson and Allison (5), in a study of the effect of visual exposure on the rate and reliability of stylus-maze learning, found that 5 seconds of visual exposure increased the reliability of the maze used from .42±.15 to .89±.04. In this experiment, the same method, using the high relief finger maze, shows the opposite tendency, viz., decreasing the reliability rather than increasing it. These results, however, are not incompatible if the assumption is valid that the difficulty of the pattern is an essential factor in determining the reliability of a maze. This probably means that the stylus maze used, when learned under com-

TABLE 1
RELIABILITY COEFFICIENTS

Group	*N*	Seconds exposed	Odds-Even
A	18	0	.90±.03
B	15	5	.71±.08

TABLE 2

CORRELATIONS BETWEEN INTELLIGENCE AND MAZE PERFORMANCE

Group	*N*	Alpha-maze trials	Alpha-maze errors	Alpha-maze time	Composite maze scores and Alpha
A	18	.33±.15	.49±.13	.78±.07	.61±.10
B	15	.49±.13	.39±.16	.18±.18	.49±.13

TABLE 3

CORRELATIONS BETWEEN PSYCHOLOGY GRADES AND MAZE PERFORMANCE

Group	*N*	Psychol.-maze trials	Psychol.-maze errors	Psychol.-maze time	Composite maze scores and psychol.
A	18	.57±.11	.21±.16	.53±.12	.50±.12
B	15	.53±.13	.41±.15	.45±.15	.52±.13

plete blindfold conditions, was too difficult to be a good measure of individual ability, while the high relief finger maze was not difficult enough, when learned by the visual exposure method, to differentiate individual ability (see Table 4).

Table 2 shows the relation between intelligence as measured by the Army Alpha test and performance on the high relief finger maze as described above. When the average of the ranks on trials, errors, and time on the maze are correlated with Alpha scores, we get .61±.10 for Group A and .49±.13 for Group B, the difference being .12±.04. Table 3 shows the correlations between psychology grades, for a twelve-weeks' course, and performance on the high relief finger maze. The reliability of the psychology grades (.84±.03) was obtained by correlating the average points made by each student each week during the first six weeks with a similar average made for the second six weeks. No other data have been reported, so far as I am aware, on the relation between school grades of any kind and the maze learning ability of adults. While the correlations in Table 3 are not high, they are all positive and those between composite maze scores and psychology are about as high as the average correlations between the better intelligence tests and school grades.[2] Changing the difficulty of our maze by the visual exposure method did not seem to affect its relation to psychology grades, probably because the difficulty factor was

[2]Jordan (2), in a study of the validation of intelligence tests, reported a median correlation of .415 between Alpha scores and university and normal school marks. This median was of 35 correlations within a range of from .22 to .67. He also found the correlation between intelligence and learning tests to be uniformly low, being grouped around .20 (p. 422).

TABLE 4

THE EFFECT OF VISUAL EXPOSURE ON ECONOMY IN LEARNING

Group	N	Sec. exposure	Trials			Errors			Seconds		
			Mean	σ	$D/\sigma_{diff.}$	Mean	σ	$D/\sigma_{diff.}$	Mean	σ	$D/\sigma_{diff.}$
A	18	0	32.72	6.38		238.11	65.11		2166.22	935.95	
B	15	5	20.12	6.35	5.83	124.12	48.85	5.97	1264.27	583.27	3.37

not controlled. That the difficulty of the maze was appreciably decreased by visual exposure is evident in the results shown in Table 4. The difference of the means is over five times the σ of their difference as judged by the criterion of trials and errors and over three times when judged by the number of seconds required for learning.

Changing the difficulty of the maze by 5-sec. visual exposure resulted in a decrease of the reliability from .90±.03 to .71±.08, a difference of .19±.04, and also a lowering of the correlation between intelligence test scores and maze-learning ability from .61±.10 to .49±.13, a difference of .12±.04. The relation between maze performance and psychology grades was not appreciably affected by the change in difficulty, the correlation being .50±.12 for Group A without, and .52±.13 for Group B, with usual exposure.

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THE LABORATORY COURSE IN PSYCHOLOGY: II. AN ANIMAL-LEARNING EXPERIMENT[1]

MILES A. TINKER

The unsatisfactory state of the elementary course in laboratory psychology is almost universally recognized. Ordinarily, the course arouses neither enthusiasm nor even interest in students. On the contrary, the students' attitude is tolerance for a requirement which frequently turns into ennui.

A survey of the texts designed for laboratory courses in psychology reveals in most instances a rather rigid adherence to traditional psychology with slight endeavor to harmonize content with contemporary trends. There are a few exceptions,[2] however, in which an attempt has been made to place laboratory psychology on a more adequate foundation.[3]

Any laboratory course which does not involve training in scientific method has a questionable objective. Experimental psychology should be made as interesting as possible without departing at all from sound psychological principles and scientific method. Moreover, such a course should incorporate experiments closely related to contemporary psychologial trends.

With the above aims in view, an experiment in animal learning was organized for use in the elementary laboratory. Marked success of the experiment over a period of two years at the University of Minnesota justifies a brief description and a citation of its merits.

The experiment consists of a maze learning situation for white rats. The apparatus employed is a vertical maze whose front view is shown by the schematic drawing in Figure 1. Outside measurements of the maze are 34½ x 4½ x 20 inches. Its front is covered with black window screen and the whole inside of the apparatus is painted black.[4] The maze is placed on a table at the front of the classroom. The black background of the apparatus enables every member of any large class to see distinctly all behavior of a white rat. When an animal enters the cul-de-sac (*C*) or progresses toward the end of any level so that its entire head appears beyond points indicated by the dotted lines, it is arbitrarily considered that an error is made.

The animal is admitted to the lower level through a door (*E*) at the left side, and progresses upward from level to level by means of the inclined planes (*I*). Retracing is prevented by lowering a trapdoor (*D*) as soon

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[1]The first articles of this series appeared in this journal (4).

[2]See (1, 2, 3).

[3]The evolution of methods and content in laboratory psychology will be considered in a later paper.

[4]A diagram drawn to scale and detailed directions for construction of this maze are given in (1, pp. 379-380). This type of maze was built by Professor A. G. Bills at the University of Minnesota in 1927.

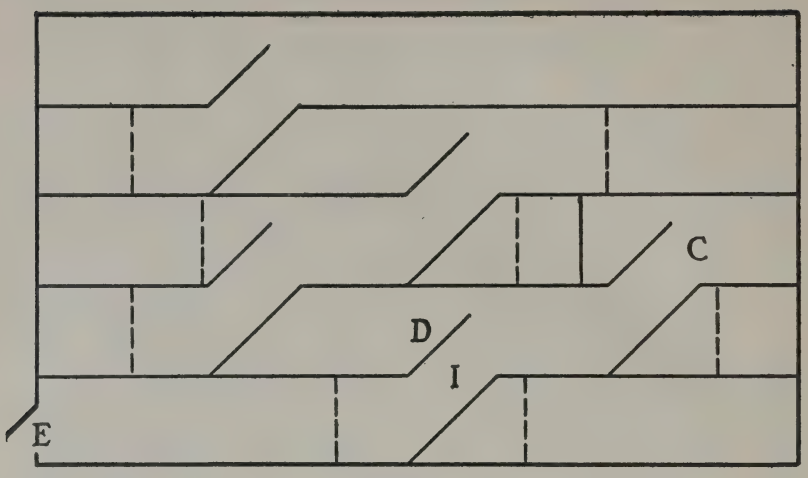

FIGURE 1
VERTICAL ANIMAL MAZE

as each level is attained. The rat finds food on the upper level where it has been fed several times prior to beginning the experiment. Timing the trials is done by the experimenter and counting errors and observation of behavior by students. Two trials per day (one trial on the first day) for approximately one week yields material which demonstrates to good advantage the learning of this situation by rats.

Times and errors for a group of 21 animals and for a rat exhibiting typical performance in the vertical maze are given in Table 1. All these data were collected in the regular laboratory situation with student observers present. Inspection of these results reveals that: (*a*) curves plotted from average times and also from individual times, would produce characteristic negatively accelerated learning curves; (*b*) curves from the error scores would be somewhat similar but show less consistent and less marked improvement; (*c*) time is a more adequate measure of learning than errors in this maze; and (*d*) this maze situation is not completely mastered in 17 trials. (In fact, four animals run for more than 40 trials still committed an occasional error. Probably the criterion for errors is too severe. Errors in cul-de-sac *C* and at the right end of the fourth level are most persistent. Seventeen trials are sufficient, however, to yield adequate data for the laboratory experiment.)

Special merits of this experiment are: (*a*) It stimulates a keen and sustained interest in the student. (*b*) It produces an adequate demonstration of animal learning. (*c*) It illustrates well the value and need of controls in animal experimentation.

TABLE 1

PERFORMANCE OF RATS IN THE VERTICAL MAZE

The average times (in sec.) and the average numbers of errors per trial for 17 trials are given for a group of 21 white rats. For comparison the times and errors of an animal yielding typical performances are also included.

	Trial No.								
	1	2	3	4	5	6	7	8	9
Av. time—21 rats	257	99	85	67	56	43	49	33	35
Time—Rat A	341	93	42	33	42	38	28	22	29
Av. errors—21 rats	13	7	6	5	4	5	4	4	3
Errors—Rat A	19	5	3	3	4	4	3	3	4

	Trial No.							
	10	11	12	13	14	15	16	17
Av. time—21 rats	42	27	27	21	23	18	18	15
Time—Rat A	12	10	9	8	11	13	10	9
Av. errors—21 rats	4	3	3	2	2	2	2	2
Errors—Rat A	1	1	1	0	2	1	1	1

This vertical maze may also be employed advantageously in general psychology to demonstrate various stages in the learning process. Exploratory behavior occurring in the first trial, an intermediate state in mastering the maze, and efficient running through the true path may be shown by exhibiting three animals, each with a different amount of training.

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